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ABSTRACTS

Politics, Pressure and Economic Policy

Explaining Japan's use of economic stimulus policies

By DENNIS PATTERSON and DICK BEASON

While supplementary budgeting has long been part of the Japanese fiscal cycle, substantive and procedural aspects of the process have changed. First, since the late 1970s, supplementary budgets have been used to fund government economic stimulus efforts (*keizai taisaku*), and second, since the late 1980s, these budgets have been assembled several months after the announcement of the actual stimulus packages. Such stimulus policies do not fit the prevailing model of the Japanese electoral business cycle, which emphasizes the targeting of benefits by the Liberal Democratic Party (ldp) at its constituents at election time. This article addresses this anomoly by developing a theory of how governing parties use the economic policy process to serve their electoral interests, particularly through broadly gauged policies designed to improve macroeconomic conditions. The authors amend the prevailing model to allow an adequate test of their electoral theory to be conducted. The results suggest that Japanese economic stimulus policies were the result of governing parties' attempts to expand their support at election time and to satisfy U.S. pressure to use fiscal policy to stimulate domestic demand.

The Benefits of Ethnic War

Understanding Eurasia's unrecognized states

By CHARLES KING

Within international relations, discussions about how civil wars end have focused mainly on the qualities of the belligerents (ethnicity, commitment to the cause) or on the strategic environment of decision making (security dilemmas). Work in sociology and development economics, however, has highlighted the importance of war economies and the functional role of violence. This article combines these approaches by examining the mechanisms through which the chaos of war becomes transformed into networks of profit, and through which these in turn become hardened into the institutions of quasi states. The first section offers a brief overview of current research on civil war endings. The second section outlines the course of four Eurasian wars and identifies the de facto states that have arisen after them: the republic of Nagorno-Karabakh (in Azerbaijan), the Dnestr Moldovan republic (in Moldova), and the republics of Abkhazia and South Ossetia (in Georgia). The third section analyzes the pillars of state building in each case: the political economy of weak states, the role of external actors, the mythologizing function of cultural and educational institutions, and the complicity of central governments. The concluding section suggests lessons that these cases might hold for further study of intrastate violence.

Globalization, Domestic Politics, and Social Spending in Latin America

A time-series cross-section analysis, 1973–97

By ROBERT R. KAUFMAN and ALEX SEGURA-UBIERGO

This study examines the effects of globalization, democratization, and partisanship on social spending in fourteen Latin American countries from 1973 to 1997, using a pooled time-series error-correction model. The authors examine three sets of issues. First, following debates in the literature on OECD countries, they want to know whether social spending has been encouraged or constrained by integration into global markets. Within this context, they examine the extent to which such outcomes might be influenced by two additional sets of domestic political and institutional factors discussed in work on developed countries: the electoral pressures of democratic institutions and whether or not popularly based governments are in power.

The authors show that trade integration has a consistently negative effect on aggregate social spending and that this is compounded by openness to capital markets. This is the strongest and

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most robust finding in the study. Neither democratic nor popularly based governments consistently affect overall social spending. The authors then disaggregate spending into social security transfers and expenditures on health and education. They find that popularly based governments tend to protect social security transfers, which tend to flow disproportionately to their unionized constituencies; but they have a negative impact on health and education spending. Conversely, a shift to democracy leads to increases in health and education spending, which reaches a larger segment of the population. The authors conclude by emphasizing the contrasting political logics of the different types of social spending.

Competitive Corruption

Factional Conflict and Political Malfeasance in Postwar Italian Christian Democracy

By MIRIAM A. GOLDEN and ERIC C. C. CHANG

This article studies the relationship between cartels of politicians and systemic political corruption in a democratic setting. Some electoral systems, including open-list systems of proportional representation, encourage intraparty competition for office. The authors analyze the relationship between intraparty conflict in postwar Italy's dominant political party, the Christian Democrats, and charges of malfeasance against Christian Democratic members of the Chamber of Deputies in the years between the first postwar parliamentary elections of 1948 and the end of the XI legislature in 1994, when the electoral system was substantially modified. Suspected malfeasance is operationalized as requests by the judiciary to lift parliamentary immunity in order to proceed with an investigation of a member of the Chamber of Deputies.

Results show a significant statistical relationship between intraparty conflict and alleged corruption on the part of DC deputies starting in the early 1970s. These results are interpreted to mean that the dramatic levels of political corruption observed in Italy in recent decades were in part an outgrowth of the search for campaign funds by incumbent DC members of parliament in competition with other candidates from the same party. Electoral competition with other parties did not significantly affect the extent of charges of malfeasance against DC deputies. Using maps, the authors also provide preliminary evidence that Italian corruption did not spread from south to north in a process of cultural contagion, as is commonly believed. Instead, they find, the determinants of corruption appear to be endogenous to institutions of the postwar political system.

Improving Forecasts of State Failure

By GARY KING and LANGCHE ZENG

This article offers the first independent scholarly evaluation of the claims, forecasts, and causal inferences of the State Failure Task Force and its efforts to forecast when states will fail. State failure refers to the collapse of the authority of the central government to impose order, as in civil wars, revolutionary wars, genocides, politicides, and adverse or disruptive regime transitions. States that sponsor terrorism or allow it to be organized within their borders are all failed states. This task force, set up at the behest of Vice President Gore in 1994, has been led by a group of distinguished academics working as consultants to the U.S. CIA. State Failure Task Force reports and publications have received attention in the media, in academia, and from public decision makers. The article identifies several methodological errors in the task force work that cause its reported forecast probabilities of conflict to be too large, its causal inferences to be biased in unpredictable directions, and its claims of forecasting performance to be exaggerated. However, the article also finds that the task force has amassed the best and most carefully collected data on state failure to date, and the required corrections provided in this article, although very large in effect, are easy to implement. The article also demonstrates how to improve forecasting performance to levels significantly greater than even corrected versions of its models. Although the matter is still a highly uncertain endeavor, the authors are nevertheless able to offer the first accurate forecasts of state failure, along with procedures and results that may be of practical use in informing foreign policy decision making. The article also describes a number of strong empirical regularities that may help in ascertaining the causes of state failure.

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POLITICS, PRESSURE, AND ECONOMIC POLICY
Explaining Japan's Use of Economic Stimulus Policies

By DENNIS PATTERSON and DICK BEASON*

INTRODUCTION

THROUGHOUT the postwar period, supplementary budgets (*hosei yosan*) have been a regular feature of the Japanese budgeting system. These budgets, which are typically adopted once per annum[1] late in the calendar year[2] by a parliamentary act outside of the nation's annual budget cycle, have often been prepared and adopted to make up for unanticipated shortfalls in tax revenue and to ensure sufficient funding for items specified in the original budget. Supplementary budgets have also been enacted to provide typhoon-related disaster relief, to reduce the national debt, and to underwrite other initiatives deemed appropriate by the Diet.

Despite the manifest regularity of Japanese supplementary budgeting, the process has undergone both substantive and procedural changes. The primary substantive change has been that from time to time—but with increasing frequency in last ten years—supplementary budgets have been enacted to fund economic stimulus efforts.[3] This initially involved no special announcement—when a supplementary

* An earlier version of this article was presented at the annual meeting of the Japan Studies Association of Canada in Halifax, Nova Scotia, October 1996. We are grateful to Jim Granato, Brad Richardson, Nic van de Walle, and several anonymous reviewers for helpful comments.

[1] There are examples of as many as three supplementary budgets being passed in a single calendar year. For a discussion of supplementary budgeting practices in Japan, see John C. Campbell, *Contemporary Japanese Budget Politics* (Berkeley and Los Angeles: University of California Press, 1977).

[2] While there have been exceptions, most supplementary budgets have been enacted in the last three months of a calendar year or in January of the following year.

[3] The key phrase here is *keizai taisaku* (economic counterpolicies). Sometimes this phrase has been used alone in reporting on such policies, but more often it has been prefaced with the words *sogo* (comprehensive) or *kinkyu* (emergency).

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budget was enacted, the media simply reported that in addition to disaster relief or some other normally funded item, the supplementary budget was intended to stimulate domestic economic activity.[4] The primary procedural change occurred after 1986. Although the government continued to propose promoting domestic economic activity with a stimulus package of a certain yen value, it then left it to the Diet to adopt a supplementary budget that determined how much would be actually spent.

Since May 1987, when Prime Minister Nakasone announced that he would propose a special package of spending to increase domestic demand, the Japanese government has announced twelve such economic stimulus packages that had to be included in subsequent supplementary budgets in order to be implemented. The dates and amounts of these twelve packages are presented in Table 1 with the announced value of each presented in column 2 (our attempt to determine how much was actually spent is presented in column 3).[5] Similar attempts to measure actual expenditures were made by the popular business press and by financial market analysts at the time,[6] but the methodology of such analyses remains unclear. Since our goal was to remove previously announced spending and nonexpenditure items from the announced figures, we have simply excluded allocations from the Fiscal Investment Loan Plan (FILP)[7] and any previously budgeted items from the estimate of hard spending.[8]

It is immediately apparent from the data that the amounts actually spent were consistently lower than the announced amounts. This may be attributable to the strong opposition of upper-level bureaucrats, es-

[4] The best examples were the four supplementary budgets enacted to carry out economic stimulus and other activities in 1977 and 1978. See Asahi Shinbun, *Asahi Nenkan* (Japan Yearbook) (Tokyo: Asahi Shinbunsha, 1978 and 1979).

[5] Although Table 1 contains only those stimulus packages announced since May 1987 inclusive, our attempt to explain the timing of the packages also includes the four packages enacted in the late 1970s.

[6] When we say popular business press, we are generally referring to such publications as the economic journal *Nihon Keizai Shinbun* and Japan's three principal daily newspapers—*Asahi Shinbun*, *Yomiuri Shinbun*, and *Mainichi Shinbun*.

[7] The FILP, Fiscal Investment Loan Plan, is essentially an extrabudgetary system of finance available to the government that utilizes funds collected through the country's Postal Savings System. For a discussion of the FILP and other aspects of the Japanese financial system, see Yoshio Suzuki, *The Japanese Financial System* (Oxford: Clarendon Press, 1987).

[8] While this methodology produces numbers comparable to the "consensus" understanding of the value of the economic stimulus packages, it still somewhat overstates the magnitude of many of them. Perhaps the best illustration of this is the September 1995 package. When loan allocations and previously budgeted land purchases are netted from the ¥14 trillion total, ¥9 trillion of expenditure remains. Of this amount, ¥1.1 trillion was earmarked for Uruguay Round countermeasures, but it was not made clear whether this would be allocated to expenditure or loan programs. Depending upon the outcome, the calculated total of "hard" expenditure would be between ¥7.9 and ¥9.0 trillion.

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TABLE 1
JAPAN'S FISCAL AND STIMULUS PACKAGES (1987–98)

Date Announced	*Announced Amount (Trillions of Yen)*	*Actual Economic Stimulus Expenditure (Trillions of Yen)*
May 1987	—	2.0
August 1992	10.7	8.6
April 1993	13.2	10.6
September 1993	6.3	5.1
February 1994	15.2	7.2
February 1995	1.6	1.6
April–May 1995	2.7	2.7
September 1995	14.0	7.9–9.0
December 1996	2.7	2.7
December 1997	15.85	2.85
April 1998	16.6	NA
November 1998	24.0	NA

SOURCES: *Asahi Nenkan* (fn. 4, various years).

pecially those in the Ministry of Finance (MOF). Since the 1970s MOF bureaucrats have been primarily concerned with the solvency of the government's fiscal position.[9] In April 1993, for example, when the Miyazawa government announced its second economic stimulus package, MOF vice minister Ozaki Mamoru stated that "[t]he Ministry of Finance 'will resist to the end' going into heavy debt again."[10] Moreover, many bureaucrats believed that such policies would be ineffective and possibly counterproductive.[11] Much journalistic reporting on the stimulus packages also came to reflect a growing skepticism over whether such policies would have any positive impact on the real economy.[12] The use of economic stimulus policies in Japan is also intriguing because after the Kennedy-Johnson packages of the 1960s in the U.S.,

[9] For an illustrative discussion of efforts by the Ministry of Finance to reduce budget deficits in the 1970s, see, for example, Yukio Noguchi, "Public Finance," in Kozo Yamamura and Yasukichi Yasuba, eds., *The Political Economy of Japan*, vol. 1 (Stanford, Calif.: Stanford University Press, 1987).

[10] See Louise do Rosario, "Giving It Back: Declining Economy Sparks Debate on Tax Cuts," *Far Eastern Economic Review* (March 11, 1993), 13.

[11] For evidence of the neutrality of Japan's economic stimulus policies, see Dick Beason, "Neutrality of Fiscal Packages during Japan's Heisei Recession" (Manuscript, Department of Business and Economics, University of Alberta, 1996). The only amendment we should add to this concerns the 1998 package, which some bureaucrats in the Ministry of International Trade and Industry publicly supported. See Peter Landers and Trish Soywell, "The Big Gamble: Will Keynesian Medicine Work for Japan and China?," *Far Eastern Economic Review* (November 26, 1998), 98–99.

[12] See, for example, Henry Sender, "More of the Same," *Far Eastern Economic Review* (October 5, 1995).

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the governments of advanced industrial societies largely abandoned economic stimulus tools.

The purpose of this article is to explain the timing of the adoption of economic stimulus policies, specifically, to identify factors that help explain why the economic stimulus packages were announced when they were. Since the packages were largely opposed by members of Japan's most important ministries and their economic value had many more detractors than supporters, one wonders why they were announced at all.

First, our explanation will show that electoral politics was the driving force behind the Japanese government's announcement of economic stimulus packages. This line of inquiry contravenes the revisionist view of Japanese political economy but it accords well with recent scholarship on economic policy in Japan, especially the model of the electoral business cycle.[13] Second, we will show that Japan used fiscal stimulus policies as a foreign policy tool, specifically as a response to *gaiatsu* (foreign pressure)—most often from the United States—to promote the importation of more U.S. goods, but recently and more pertinent to this analysis, to stimulate its own failing economy. Many have shown *gaiatsu* to be important,[14] but none have included it in a more general model of the Japanese political economy. We begin this effort by examining the scholarship that informs our understanding of economic policy-making in postwar Japan.

Economic Stimulus Policies and the Electoral-Business Cycle in Japan

Even scholars who believe the power to make economic policy in Japan rests with the nation's bureaucrats acknowledge that "political surfing" has occurred throughout the postwar period. Political surfing refers to an incumbent government setting the time of an election for when the

[13] For the revisionist view of economic policy-making in Japan, see Chalmers Johnson, *MITI and the Japanese Miracle* (Stanford, Calif.: Stanford University Press, 1982). Perhaps the best general statement of the influence of electoral politics on policy-making in Japan can be found in Kent Calder, *Crisis and Compensation: Public Policy and Political Stability in Japan, 1949–1986* (Princeton: Princeton University Press, 1988). Other scholarly work we refer to includes, on budgeting, Matthew McCubbins and Greg Noble, "The Appearance of Power: Legislators, Bureaucrats, and the Budget Process in the United States and Japan," in Peter Cowhey and Matthew McCubbins, eds., *Structure and Policy in Japan and the United States* (New York: Cambridge University Press, 1995); and more generally on policy-making in Japan, Mark Ramseyer and Frances Rosenbluth, *Japan's Political Marketplace* (Cambridge: Harvard University Press, 1993).

[14] See Leonard Schoppa, "*Gaiatsu* and Economic Bargaining Outcomes," *International Organization* 47 (Summer 1993).

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macroeconomy is performing well in order to enhance its candidates' electoral prospects.[15] This idea has recently been amended by Masaru Kohno and Yoshitaka Nishizawa, who demonstrate that electoral politics and economics in Japan are connected in a way that goes beyond political surfing.[16] They argue that incumbent governments face a number of constraints significant enough to prevent them from simply calling an election any time they consider electoral conditions to be amenable. Kohno and Nishizawa do not deny the occurrence of political surfing, but they emphasize that given the constraints elected officials face, they also engage in a "targeted manipulation of specific economic programs." Their analysis of the thirty-year period from 1955 to 1985 reveals that actual spending for construction projects in Japanese election districts increased significantly during months in which general elections were held.

This result is significant for our purposes, because while Kohno and Nishizawa find a correlation between elections and the expenditure of public monies on constituency construction projects, they also find that elections cannot explain in a statistically significant way the actual budget allocations of public works monies. They explain that compared with actual construction spending, budget allocations are much more diffusely targeted and thus it is unclear who their direct beneficiaries are to be.[17] They then conclude that the strength of the LDP's "patron-client bonds" in Japanese election districts requires that government largesse be targeted directly at the party's constituencies at election time.

This finding is significant because the economic stimulus measures examined in this article rarely entail actual construction spending; more often they are similar to the budget allocations that Kohno and Nishizawa regard as statistically insignificant in their model. Indeed, our stimulus packages involved uncertainties because the nature of the

[15] The reason that most scholars accept the "political surfing" view is that while it conforms to the dominant politician view, it does not at the same time conflict with the view that Japanese bureaucrats are autonomous in the area of economic policy-making.

[16] See Masaru Kohno and Yoshitaka Nishizawa, "A Study of the Electoral Business Cycle in Japan," *Comparative Politics* 22 (January 1990). It is true that Alesina and Roubini also found evidence of the "political business cycle" in Japan that went beyond political surfing. See Alberto Alesina and Nouriel Roubini, "Political Cycles in OECD Economies," *Review of Economic Studies* 59 (October 1992). However, their model tested the impact of elections on economic outcomes—namely, GDP growth and unemployment—not on the impact that elections had on the probability that a certain economic policy would be announced.

[17] Kohno and Nishizawa also pointed out that the lack of a statistically significant relationship may be due in part to the difficulties of specifying such a model correctly. See Kohno and Nishizawa (fn. 16), 159–90.

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expenditures in each package was rarely announced with any specificity, and the amounts ultimately spent fell short of those originally announced. This made it very difficult to identify who would be the packages' direct beneficiaries and to calibrate the specific benefits they were to receive. The problem then is that if the Kohno-Nishizawa model completely subsumes the Japanese economic policy-making process, there is no reason to expect electoral politics and *gaiatsu* to be important explanatory factors in the announcement of the economic stimulus measures we examine.

To address this problem, we argue that the model needs to be amended because Kohno and Nishizawa assume many things about the relationship between electoral politics and economic policy that are either untenable or at least require amendment because of changes that occurred throughout the postwar period. We agree with them that the LDP's patron-client bonds may have required the party to target government largesse directly at its constituents at election time, but we question the implication that targeting only constituency supporters has always been sufficient to help the LDP gain an electoral majority. At several times during the last two decades, the LDP's majority was either very slim or achieved only through the assistance of non-LDP politicians; therefore it is difficult to imagine the party not endeavoring to use the policy process to attract new supporters.[18] Nonetheless, Kohno and Nishizawa most likely make this assumption because they also assume that a strong macroeconomic performance "may *not* be important in the minds of politicians and voters in Japan," since a strong economy may not benefit the incumbent party at election time. They add that "[o]ne might even argue that poor economic conditions will favor the LDP since the people will become cautious and avoid an adventurous choice in selecting a party to vote for."[19]

While we owe much to the work of Kohno and Nishizawa, we find it necessary to abandon their assumption that macroeconomic conditions in Japan do not work electorally the way they do in other advanced societies. Our analysis is based on the idea that both the declining Liberal Democratic Party that controlled the government in the late 1970s with difficulty and the rather weak parties and coalitions

[18] In the early 1960s the leadership of the LDP made it known that it would soon face a support crisis because of demographic changes that were taking place in the country. In a 1963 *Chuo Koron* (Central Review) article, LDP member Ishida Hirohide stated that socioeconomic changes brought about by Liberal Democratic Party policies would lead to conservative-progressive electoral parity unless the LDP began to attract new supporters. See Fukashi Horie and Masahiko Umemura, *Toyo Kodo to Seiji Ishiki* (Voting behavior and political consciousness) (Tokyo: Keio Tsushinsha, 1986), chap. 1.

[19] Kohno and Nishizawa (fn. 16), 163.

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that controlled the government from 1993 to the present needed to be perceived as being committed to strengthening the Japanese economy.[20] We further amend the Kohno-Nishizawa model by factoring in the role of U.S. pressure in encouraging the Japanese government to use fiscal policy to stimulate domestic demand. We believe that *gaiatsu* is important, first, because the U.S. eventually demanded that Japan implement domestic stimulus policies, and second, because successive governments had little to lose by giving in, since such policies would both placate the U.S. and serve the electoral interests of the LDP and the other parties that governed during the period under consideration. We elaborate on these two amendments to the model of the Japanese electoral-business cycle in the following two sections.

Turnout, Support Expansion, and Economic Policy

The Liberal Democratic Party, from its formation in fall 1955 until the mid-1970s, singularly controlled every government. Between the general election of 1976 and the contest of summer 1986, the LDP maintained control of the Japanese government, but often with the assistance of unaffiliated conservative politicians and the New Liberal Club.[21] The LDP fared well until the general election of 1993, when a coalition of old and new parties and unaffiliated candidates pushed it out of power for the first time in thirty-eight years.[22] The LDP lost its electoral predominance in both the last half of the 1970s and the 1990s.[23] This is critical because the level of a governing party's majority determines the imperatives it faces at election time, and different electoral imperatives affect how a governing party is persuaded to use the tools of economic policy to serve its electoral interests.[24]

When a single party holds a stable, robust majority in the lower house of a legislature, its principal challenge at election time is best re-

[20] To serve its electoral interests the LDP used not only fiscal policy but also other policy tools. For a general discussion of how the ruling party used the policy process to solve the numerous electoral crises it faced throughout the postwar period, see Calder (fn. 13).

[21] The New Liberal Club was formed in 1976 by several reform-minded members of the LDP. It put up candidates in the next five lower-house elections but rejoined the LDP after its disappointing performance in the double election of 1986.

[22] The LDP returned to power less than a year later, either as a minority party or in coalition with some other party or individuals. In terms of its underlying electoral strength and the strength and unity of the opposition it faces, it is not the same party it was earlier in the postwar period.

[23] The LDP is currently the governing party, but in coalition with other parties, as in the recent past. It still has not recovered from its loss in 1993.

[24] This is not to suggest that a governing party with a majority is uninterested in expanding its level of support. Rather, it is to emphasize that governing parties with sufficient support to obtain a majority have to be concerned more with their core supporters turning out and voting than with attracting new supporters.

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ferred to as a turnout imperative. The party's electoral strategy is focused less on expanding its support base (because it already possesses sufficient support to win the election and retain its majority) than on ensuring that its core supporters go to the polls on election day. By contrast, governing parties that *do not* possess a majority of lower-house seats at election time face a support expansion imperative—they must expand their support levels to obtain a majority of seats in the legislature. Even if a political party has no chance of obtaining a majority, it still has incentive to try to increase its share of seats and thereby increase its bargaining power within the government (or coalition if this be the case).[25]

Knowing whether governing parties face a turnout or a support expansion imperative at election time indicates how likely they are to use the tools of economic policy—particularly policies designed to improve macroeconomic conditions—to serve their electoral interests.[26] Despite the limitations of their model, Kohno and Nishizawa advanced this line of inquiry. They showed that spending for actual construction projects increased dramatically during months in which elections occurred because for much of the postwar period the LDP faced largely a turnout imperative at election time and needed to remind its supporters that it had been their benefactor party and deserved their support. A strategy involving direct transfers, such as spending on construction projects as close to the election as possible, is undoubtedly better suited for this purpose than more broadly gauged efforts designed to improve macroeconomic conditions.

But how does a governing party's election behavior change when a turnout imperative strategy alone is insufficient to obtain electoral victory? Governing parties without a majority must attract new supporters at election time in addition to ensuring the participation of their core supporters. Since voters available for mobilization are generally individual members of a diffuse mass public, the most effective strategy would be to use broadly gauged, public goods–type policies designed to improve macroeconomic conditions. Our expectation, therefore, is that when governing parties face a support expansion imperative at election time, they will follow a strategy of demonstrating their commitment to

[25] The motivation for this would simply be more portfolios or a greater share of the most important portfolios, given that the distribution of portfolios tends to reflect the party's distribution of seats. See, for example, Eric Browne and Mark Franklin, "Coalition Payoffs and European Parliamentary Democracies," *American Political Science Review* 67 (June 1973).

[26] This division may be somewhat misleading because governing parties with a weak or nonexistent majority actually face both imperatives simultaneously. However, parties that need to attract additional support must still be sure that their core supporters turn out and vote.

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strengthening the macroeconomy. Accordingly, we should have expected the LDP to consider policies to improve macroeconomic conditions during periods in which its predominance was either threatened or lost. Since the economic stimulus policies just discussed were announced and enacted in exactly such periods, we are confident that electoral politics will be significant—both substantively and statistically—in our amended model.

Foreign Pressure (*Gaiatsu*) and Economic Stimulus Policies

One would expect *gaiatsu* to play a role in Japan's use of economic stimulus policies since it has been so prevalent in U.S.-Japanese relations—indeed it has been perhaps the defining characteristic of the history of U.S.-Japanese relations. It was U.S. pressure that ended the policy of isolation implemented by the Tokugawa Shogunate for nearly two and a half centuries. *Gaiatsu* from the U.S., while ultimately ineffective without the use of military force, was employed to halt Imperial Japan's expansion into China and Southeast Asia. After the Second World War, in an attempt to make Japan a peaceful and democratic nation, U.S. occupation authorities pressured early postwar governments into making numerous reforms, including the postwar constitution. Finally, the U.S. has used *gaiatsu* throughout the entire postwar period to bring about many changes in its economic relations with Japan.

However, the prevalence of *gaiatsu* does not demonstrate how it works as an explanatory variable. To achieve that end we must answer two questions: first, why would the Japanese government respond to U.S. demands to stimulate its domestic economy, and second, how did *gaiatsu* affect the timing of the government's economic policy announcements? By the mid-1980s the U.S. had already been engaged in negotiations with Japan for some time in an effort to reduce its bilateral trade deficit. U.S. negotiators, believing that much of the problem rested with reputably producer-friendly Japanese economic policies that suppressed consumer demand, pressured the Japanese government to stimulate domestic demand. This type of *gaiatsu* found its clearest articulation in U.S. demands that the Japanese government implement recommendations contained in the two Maekawa Reports that were prepared in the mid-1980s.[27] From the publication of these reports to

[27] These were prepared by the Advisory Group on Economic Structural Adjustment for International Harmony, a group appointed by Prime Minister Nakasone and chaired by former Bank of Japan governor Maekawa Haruo. For a discussion of specific proposals contained in the reports, see Bela Balassa and Marcus Noland, *Japan in the World Economy* (Washington, D.C.: Institute for International Economics, 1988).

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the present day, the U.S. has continually encouraged the Japanese government to stimulate domestic economic activity (albeit for somewhat different reasons).[28]

There is little doubt that the Japanese leadership and citizenry wanted good relations with the U.S. and knew that acceding to U.S. demands would serve this end. However, the history of U.S.-Japanese relations is also defined by periods of intransigence. In his study of the differential success of U.S. pressure in getting the Japanese government to agree to demands made in the Structural Impediments Initiative (SII) talks, Leonard Schoppa showed that *gaiatsu* from the U.S. worked in some cases but not in others. He correlates this differential success rate with the ability of the U.S. to mobilize those groups that would benefit from the Japanese government following U.S. demands. In the case of the economic stimulus policies we are examining, the governing parties themselves would be among the beneficiaries. Thus, as long as governing parties faced a support expansion imperative and economic stimulus policies could be used to attract new support, acceding to U.S. demands would coincide with the electoral interests of Japan's governing parties. However, Schoppa's findings do not tell us when governing parties would be most likely to time the announcement of such economic stimulus policies.[29]

Negotiations between the United States and Japan on this and other economic issues were intermittently punctuated by meetings between the U.S. president and the Japanese prime minister to discuss problems in the relationship and affirm their mutual commitment to solve outstanding problems. Although these gatherings were more often media events than substantive discussions, they were nonetheless important, since on those occasions the leaders felt compelled to announce the achievement of important compromises or some common understanding aimed at reducing tensions over economic differences. Consequently, if there were ever a time when the Japanese government would be likely to announce an economic stimulus policy in response to U.S. pressure, such a gathering would be it. Prime Minister Miyazawa's visit to the U.S. in April 1993 is illustrative of this point. Because trade and economic problems had dominated the agenda of U.S.-Japanese rela-

[28] As previously stated, U.S. policymakers originally encouraged economic stimulus policies to increase domestic demand in Japan and thereby increase the flow of U.S. exports. While U.S. policymakers have long desired domestic economic stimulus policies, they want them now even more because of the depressed state of the Japanese economy.

[29] See Schoppa (fn. 14).

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tions for some time, he was under strong pressure to report some progress on the issue when he met with President Clinton.[30]

A Test of the Amended Model

Our next task is to provide an empirical test of our amended model to determine if the announcements of economic stimulus policies were timed to serve the electoral interests of governing parties and at the same time to appease the United States. We expect both electoral politics and *gaiatsu* to be important explanatory factors as long as governing parties faced support expansion imperatives. Not only would such a condition encourage governing parties to use the instruments of economic policy to attract additional supporters at election time, but it would also allow the Japanese government to acquiesce to U.S. demands to stimulate its economy, because such an action would coincide with its electoral interests. Due to these expectations, we define the dependent variable in the amended model as a monthly series that begins in January 1975 and ends in December 1998. For this period, we coded a month as 1 if the government announced an economic stimulus package either as part of a supplementary budget, as it did several times at the end of the 1970s, or separately without any reference to a supplementary budget, as it did in the past decade. All other months in the series are coded as 0.

We realize that using a series that covers the period from 1975 to 1998 raises some problems because there was a change in the way supplementary budgets related to the fiscal stimulus policies we seek to explain. Nonetheless, we believe the first four packages must be kept in the analysis: while they were announced differently than the later packages, they still constitute the same kind of policy and thus should be explainable by the same factors. Nonetheless, to be sure that we account for any impact that the structural break identified in the beginning of the article may have had on the process, we adjust our analysis in two ways. First, we include a variable to capture the structural break that occurred in the process in our initial runs, and second, we conduct the analysis separately for the post-1986 period and compare the results with those obtained for the extended series.

[30] The economic stimulus package corresponding to the April 1993 entry in Table 1 was announced by Prime Minister Miyazawa at his meeting with President Clinton.

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Independent Variables: Electoral Politics

If policies designed to improve macroeconomic conditions are motivated by a government's desire to attract additional electoral support, then the constitutional and political processes that define democratic politics in the Japanese parliamentary system—including elections—should tell us something about the timing of such policies. Elections, like a snapshot in time, produce an unretouched picture of the level of support governing parties maintain and determine their capacity to govern. Thus, when an election is called, we would expect governing parties to use economic stimulus policies to help them retain control of the government.

While this is a realistic expectation, it does not tell us exactly when electorally motivated policy announcements should be made. Kohno and Nishizawa found that constituency spending on construction projects was highest during months in which elections were held. As previously discussed, the LDP addressed the turnout imperative in such instances by directing public money to its core supporters as close to the election as possible to remind them to turn out and vote. When, however, governing parties face a support expansion imperative—for which policies that improve macroeconomic conditions are appropriate—a different timing strategy is necessary.

Governing parties responding to a support expansion imperative face a large and generally undifferentiated group of politically unattached voters. To capture such individuals at election time, governing parties' primary need is to demonstrate their ability to develop and implement policies that address salient issues.[31] When policies designed to improve macroeconomic conditions are appropriate for such an effort, governing parties need to announce the policy close enough to reap the political benefits but not so close as to appear overly political. Announcing an economic stimulus policy during the same month an election is held would open the governing party to charges that its motives were purely political; conversely, announcing an economic stimulus policy too far in advance of an election could prevent the governing party from reaping the electoral benefits that such an announcement is designed to pro-

[31] By salient issues, we mean those problems that define the agenda of an election. This concept helps define the critical distinction that exists between a party's core supporters, who generally vote in response to partisan or long-term cues, and those potential supporters it wishes to add to its rolls (peripheral voters), who vote more in response to election-specific, short-term cues. For a detailed discussion and empirical tests of this idea, see Ian Budge and Denis Farlie, *Explaining and Predicting Elections* (London: George Allen and Unwin, 1983); and John Petrocik, "Issue Ownership in Presidential Elections with a 1980 Case Study," *American Journal of Political Science* 40 (August 1996).

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vide. For these reasons, we include general elections as a dummy variable in our amended model, but enter it with a two-month lag.[32]

It is necessary to point out here that during the period of time examined in this article, the Japanese government has had some control over both the timing of elections and the fiscal policies we seek to explain. At least to some extent then, one agent controls variables on both sides of the equation we later estimate, presenting us with a potential endogeneity problem. To address this issue and to be sure that our analysis does not produce biased results, we must consider how much control the Japanese government actually has over the timing of elections. Our analysis of necessity includes both upper- and lower-house elections, because while coordination has occurred and double elections have been held, elections to the two houses operate on different constitutionally prescribed schedules. The Japanese constitution stipulates that half the members of the House of Councillors be elected every three years, which significantly limits the ability of the government to control the timing of elections. Indeed, since 1956 inclusive, all fifteen House of Councillors elections have been held in the summer (June or July), and for the period covered by our analysis, upper-house contests were always held within two weeks of the preceding contest.

While successive Japanese governments had little sway over the timing of upper-house elections, the timing of elections to the House of Representatives is subject to much more government control. However, even that control has its limits—the power of governing parties to call an election to the lower house of the Diet is subject to both positive and negative constraints. The negative constraint is simply that regardless of how much control a governing party has over the timing of a lower-house election, it will be loath to call an election when its support rates are down because deleterious electoral consequences would likely ensue. Therefore, a governing party has an incentive to wait for more favorable electoral conditions. The negative constraint, though strong, is counterbalanced by the positive constraint—that governing parties cannot wait indefinitely for their support rates to rise before calling an election to the lower house. This is because the Japanese constitution

[32] For the purposes of our amended model, both lower-house (House of Representatives) and upper-house (House of Councillors) contests are included. We realize that the House of Councillors is less powerful than its counterpart but note that its control has nonetheless been extremely important if not essential for the success of any governing party's legislative program during much of the period we considered. It is difficult for us to imagine that it would be ignored electorally, and for this reason we included such elections in our analysis. See the appendix for the dates of all general elections held during the period under consideration, and see also fn. 48 for a discussion of the statistical significance of this variable at different lags.

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requires that elections to the lower house be called no later than four years from the date of the previous contest.

Acknowledging, however, that a governing party's ability to call a lower-house election is subject to constraints does not allow us to assume away the endogeneity problem. We therefore amend our empirical analysis in two ways. First, we control for the negative constraint a governing party faces in its attempt to call a lower-house election at a politically opportune time, and second, we control for the positive constraint on calling lower-house elections. To control for the negative constraint, we include a cabinet support variable that allows us to be sure that the elections variable itself is substantively and statistically significant and not just a proxy for a governing party seeking an electoral referendum when its support rates are high. Moreover, it also allows us to test for any independent effect support rates may have on the probability of a government using the fiscal policy process for political purposes. Specifically, when a governing party wants to continue governing through an extant cabinet but suffers from persistently low approval rates, it can use the policy process as a remedy. The cabinet support variable in our amended model will capture this.[33] The data we use for this variable come from the monthly party and cabinet support polls conducted by the Jiji News Organization (Jijitsushinsha); the results of these polls are reported one month after they are taken. We subtracted the percentage of cabinet nonsupporters from the percentage of supporters to produce an index of cabinet support with a mean value of −0.811 percent over the months contained in our series and minimum and maximum values of −79.4 percent and 55.7 percent, respectively. Decimals with negative values indicate a plurality of nonsupporters while those with positive values indicate the opposite. As it is highly unlikely that a governing party would change fiscal policy in response to a low support rating in a single month, it is our expectation that governing parties would act only after several months of consistently low approval ratings. We therefore enter the cabinet support control variable at four-, five-, and six-month lags.[34]

Controlling for the positive constraint is a more complicated task. While governments may prefer to call elections at politically opportune times, there are limits to how long they can wait. As a result, we need to differentiate between elections called well outside the constitutional

[33] The circumstances described here assume that options such as calling an election or forming a new cabinet are neither politically desirable nor procedurally feasible.

[34] See fn. 51 for a discussion of the substantive and statistical significance of this variable at different lags.

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schedule (for strictly partisan purposes) from those that are called in response to the constitutional requirement governments face every four years. To do so, we eliminate from the analysis all elections that were called more than six months before constitutionally prescribed and compare the results obtained with those obtained when all elections are included.

In addition to such constitutional processes as elections, democratic politics involves political processes that require governing parties to be interested in the levels of support they possess because support levels do more than determine whether or not a party can achieve governing status. They also reflect the ability of governing parties to put together cabinets that can govern effectively, implement their policy agendas, and avoid motions of no-confidence—which if successful can force them either to call new elections or relinquish control of the government. As previously mentioned, even though governing parties have an incentive to keep support levels high, they will not necessarily use tools of economic policy to raise approval ratings each time they drop below desired levels for a certain length of time. For one thing, the use of the policy process for electoral purposes too often can be counterproductive. Second, there is another effective method available to governing parties in Japan to build support—the ability of governing parties to form and reform cabinets. In postwar Japan this has occurred in two ways. The first is the cabinet reshuffle—the replacement of some or all ministers with others from the same party. The second involves a cabinet change that results in the formation of an entirely different government, that is, one headed by a different prime minister.

Cabinet reshuffles have occurred in postwar Japan both frequently and routinely. For the period covered by our data series, cabinet reshuffles occurred a little more than once per annum.[35] Accordingly, we have little reason to expect them to be related to the announcement of the economic stimulus packages we are attempting to explain. However, for those cabinet changes that involved the formation of a new government, our expectations are different. New governments tended to be formed when support for an existing cabinet had dropped to very low levels. Using the Jijitsushinsha party and cabinet support polls,[36] we note that one and two months prior to a cabinet reshuffle, cabinet supporters were outnumbered by nonsupporters by an average of 7.05 per-

[35] During the twenty-four-year period from 1975 to 1998 inclusive, twenty-six cabinet reshuffles occurred.

[36] Cabinet support data collected by Jijitsushinsha after 1992 were provided by the Chuo Chosasha news organization.

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cent and 5.59 percent, respectively. This suggests that new governments sought to assume power with as much support as they could acquire. During times when the macroeconomy was a salient issue, a newly formed government wanted to demonstrate that it could take decisive action. Consequently, we would expect the announcement of an economic stimulus package to follow the formation of a new government.

This expectation itself does not indicate when an economic stimulus package is likely to be announced after a new government is formed. Since an economic stimulus package is intended to increase governing party support, a new government most likely would want to assemble and announce the policy upon formation. This timing, however, might not be realistic. Since the new government was formed when support for the cabinet it replaced was low, the creation of the new government itself was the product of considerable bargaining among either LDP faction leaders (in LDP-only governments) or the leaders of those political parties that were potential coalition members. Negotiations involved not simply the issue of who would be the new prime minister but also the issue of portfolio distribution. For these reasons, we concluded that the announcement of an economic stimulus package would be unlikely to occur during a month in which a new prime minister took control of the government, and thus we enter the new government variable in our amended model at a one-month lag.[37]

Independent Variables: Foreign Pressure and Economic Conditions

As previously discussed, the other major factor responsible for the announcement of Japanese economic stimulus policies is *gaiatsu*. In our amended model, we define *gaiatsu* as the event that puts the most pressure on Japanese leaders to use the policy process to stimulate domestic demand. Such events are included in the amended model as those months in which a meeting took place between the U.S. president and the Japanese prime minister. In the period covered by our data, there were twelve instances of such meetings. Consequently, these months were assigned a value of 1 while all other months were assigned a value of 0.[38]

Because *gaiatsu* was not a factor in the first four economic stimulus packages announced in the late 1970s, we compare the impact of foreign pressure on the timing of fiscal stimulus policies both for the entire

[37] In our discussion of model results, we report the statistical and substantive significance of this variable at different lags. See fn. 49.

[38] Meetings of U.S. presidents and Japanese prime ministers are listed in the appendix.

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series and for the post-1986 period. In addition, we realize that *gaiatsu,* like the case of elections, may also present endogeneity problems, since the Japanese government has at least some control over the timing of U.S.-Japan summits. Upon closer examination, however, we conclude that the summit variable does not present the level of problem presented by elections, because the Japanese government's control over summits is not only much less thorough than it is over lower-house elections, but also because summits are timed in response to different political imperatives than elections.

The timing of summits is the result of bargaining between leaders of the U.S. and Japan, taking into account many imperatives that are in large measure quite distinct from those that drive domestic policies (for example, U.S.-Japanese relations in the areas of security, the economy, and Japan's coordination of its Asia policy with the large U.S. role there). Furthermore, the U.S. side will most likely be the more influential in determining the dates of the summits. The combination of these two factors leads us to conclude that this variable and the dependent variable it is meant to help explain are not simultaneously determined.

With electoral politics and *gaiatsu* defined, we next turn to a final control variable added to our amended model to ensure that its specification is complete. This variable is designed to capture the independent impact of macroeconomic conditions on the probability that a package would be announced. It can tell us whether the domestic and foreign political factors we have discussed are operative over and above what we would expect any incumbent government to do given the existence of certain economic problems. Our measure of macroeconomic conditions is derived from Japan's index of production (manufacturing and mining) measured as year-on-year changes—how a current month's index compares with the same month's index from the previous year.[39]

We recognize that a more well known measure such as per capita GDP may be preferable as our macroeconomic control variable. Unfortunately, GDP data are reported quarterly and thus do not conform to the structure of our data series, whereas the year-on-year changes in the production index are collected monthly and therefore are suitable for our data series. However, they are announced at a two-month lag; in combination with our desire to capture any seasonal (quarterly) adjustments that may occur and any lead time any government would need to respond to worsened economic conditions, we enter the index in our

[39] For the period under consideration, the mean value of the year-on-year changes in the production index was 2.79 while minimum and maximum values were −19.2 and 14.0 respectively.

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amended model at two-, three-, and four-month lags.[40] We feel confident using year-on-year changes in the production index for our macroeconomic control variable because they are used frequently by Japanese economic planners (including politicians) to assess the health of the economy. As such, they are an excellent indicator of trends in the Japanese economy because they signal whether the economy is moving in the correct direction and whether, in times of weak or no economic movement, positive growth in GDP is in the offing.

Amended Model: Estimation and Results

Again, given that we are interested in determining whether or not the announcement of an economic stimulus package was a function of political factors—defined as domestic electoral influences and foreign pressure—controlling for economic conditions, the model for estimation will be as follows:

$$Yt^* = \alpha + Xt \ldots t - k\beta + \mu _____ t = 1 \ldots 278 _____$$

where Y_t^* is a dichotomously valued, unobserved variable underlying the probability that an economic stimulus package will be announced, and $X_{t..t-k}$ is a one-by-nine vector containing the covariates. From the above discussion, recall that three of the covariates are dummies, indicating (1) the presence/absence of a national election (at time t-2), (2) the formation of a new government (at time t-1), and (3) an official meeting between the U.S. president and the Japanese prime minister (at time t). Three of the remaining covariates (4, 5, 6) are continuous variables measuring the level of cabinet support at lags four, five, and six, while the last three (7, 8, 9) are continuous variables gauging macroeconomic conditions, using the nation's production index measured as year-on-year changes, at lags two, three, and four.

Since the dependent variable in the amended model is dichotomous, a maximum likelihood estimation procedure (logit) is necessary. However, before discussing the model's estimation and results, we turn to the methodological issues that are presented by our amended model and data. Like linear regression, estimation of logit models assumes that observations on the dependent variable are independent of each other. For our dependent variable, the assumption of independence is violated—there is a near zero probability that economic stimulus packages would be announced two months in a row. The primary reason is that it takes too much time to both marshal the political resources nec-

[40] See fn. 51 for a discussion of this variable's estimates at different lags.

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essary to agree on an economic stimulus policy and actually prepare a package.[41]

Logit models that contain observations on the dependent variable that are temporally dependent involve the risk that parameter estimates will be inconsistent and biased. We must therefore first determine the extent to which this lack of temporal independence leads to such statistical problems that must be corrected in our estimation procedure. Following a recommendation provided by Nathaniel Beck and others, we add a temporal dummy variable to our amended model, coding the month following the announcement of an economic stimulus package as 1 and all other months as 0.[42] We then estimate this model as well as our original model, examining the impact of the temporal dummy variable on our original estimates and then determining the need to keep the temporal dummy in our amended model by employing a standard likelihood ratio test. Examining the results presented in Table 2, we see that inclusion of the temporal dummy changes the substantive significance of our estimates by only the slightest amount and the statistical significance of our estimates not at all. Also, multiplying the difference between the two models' log likelihoods by −2 is 0.330. Given that this difference follows a theoretical chi square distribution with one degree of freedom, the probability of a larger value is .5, allowing us to accept the null hypothesis of temporal independence.[43]

Next, to be certain that other statistical problems do not reduce the reliability of the results presented in Table 2, we perform other model diagnostics. The first diagnostic entails identifying cases that do not fit the model well and determining why this is the case. By producing several diagnostic plots, we can isolate five cases that do not fit.[44] In one of the cases the model predicts the announcement of a package at a high

[41] It may seem reasonable to argue that a successive package would not be announced for two or three months at a minimum and that our assumption of two consecutive months is not restrictive enough. In response, we point out that our data contain examples of package announcements two and four months apart, confirming our belief that it is nearly impossible that packages will be announced two months in a row.

[42] Nathaniel Beck, Jonathan Katz, and Richard Tucker, "Taking Time Seriously: Time-Series Cross-Section Analysis with a Binary Dependent Variable," *American Journal of Political Science* 42 (October 1998).

[43] We are grateful to Tse-Min Lin, Robert Lowery, and Mark Jones for helpful suggestions on this problem. For treatments of this problem generally, see Barry Bye and Gerald Riley, "Model Estimation When Observations Are Not Independent," *Sociological Research and Methods* 17 (November 1989); Scott L. Zegler, Kung-Yee Liang, and Steven G. Self, "The Analysis of Binary Longitude Data with Time Independent Covariates," *Biometrika* 72 (April 1985); and Simon Jackman, "Times Series Models for Discrete Data: Solutions to a Problem with Quantitative Studies of International Conflict," (Manuscript, Department of Political Science, Stanford University, 1998).

[44] The plots we examined were those pairing predicted probabilities with (1) changes in the Pearson X^2, (2) changes in deviance, and (3) changes in influence.

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TABLE 2
POLITICS, PRESSURE, AND ECONOMIC STIMULUS POLICIES:
RESULTS FROM ESTIMATING THE AMENDED MODEL

	With Temporal Dummy			*Without Temporal Dummy*		
Variables	*Estimates*	*SE*	*Z*	*Estimates*	*SE*	*Z*
Constant	−3.633**	0.472	−7.706	−3.589**	0.453	−7.911
Elections (t-2)	1.845*	0.861	2.143	1.927*	0.841	2.292
Gaiatsu						
U.S. pressure	2.513**	0.842	2.981	2.451**	0.827	2.963
New govt. (t-1)	1.712*	0.790	2.168	1.662*	0.780	2.130
Cabinet support						
(t-4)	−0.034	0.031	−1.090	−0.033	0.031	−1.052
(t-5)	0.049	0.035	1.414	0.048	0.034	1.370
(t-6)	−0.021	0.016	−1.305	−0.020	0.016	−1.299
Economic conditions						
(t-2)	0.237	0.153	1.546	0.239	0.152	1.565
(t-3)	−0.297	0.171	−1.738	−0.287	0.168	−1.703
(t-4)	0.011	1.434	0.082	−0.001	0.139	−0.014
Temporal dummy	0.525	1.238	0.425			
	Total cases 274 Log likelihood −47.728 Chi square 20.86 DF = 264; significance = 0.0012			Total cases 274 Log likelihood −47.811 Chi square 20.69 DF = 265; significance = 0.0057		

** p < .01; * p < .05.

probability (0.703), but no package was announced. Our investigation of this case shows that the reason for the high predicted probability is a cabinet support rating at time t-6 that is near that variable's minimum in our series.[45] Since our theory suggests that low cabinet support rates would lead to the use of the policy process only if they were low for several consecutive months, this case does not undermine the amended model's theoretical underpinnings.

The four remaining anomalous cases involve months for which the model predicted a low probability of an announcement of an economic stimulus package, yet one was announced. Three of these cases partially

[45] For this case, cabinet support rates were positive at lags four and five, and no other independent variables took on values that would have indicated the announcement of an economic stimulus package was a high probability.

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fit the amended model in that each involved several consecutive months of low cabinet support rates.[46] The remaining case, December 1997, represents more of an outlier because it involved low cabinet support rates, but not at the lags we specified.[47] Accordingly, we were interested to know if this case also exerts a strong influence on the results. By reviewing a plot of predicted probabilities with logit's equivalent of a Cook's D, we discover that there were no cases that were either unduly influential or that contradicted the main pattern reflected in the data. With these issues resolved, we now turn to a substantive interpretation of our results.

Discussion

From the estimates provided at the bottom of Table 2, we can say that the model is well within the range of statistical significance. Also, the large and statistically significant coefficients on the "Elections (t-2)," "*Gaiatsu*," and "New Government (t-1)" variables tell us that when we consider the Japanese government's use of economic stimulus policies, domestic politics and foreign pressure matter. With regard to national elections, the coefficient in Table 2 suggests that two months before an election is held, the probability that an economic stimulus package will be announced increases by almost seven times.[48] Furthermore, our results indicate that domestic politics increase the probability of a package being announced in another way—one month after the formation of a new government, the odds that an economic stimulus package will be announced increase by over five times.[49] Perhaps the most interesting finding is that foreign pressure increases the odds of an economic stimulus package being announced more than any other factor—a formal meeting between a Japanese prime minister and a U.S. president

[46] These were the cases for December 1977, September 1978, and May 1995.

[47] Cabinet support rates were low at times t, t-1, and t-2. It is therefore possible that our lag structure is somewhat misspecified. See the discussion in fn. 51.

[48] The decision to include national elections at a two-month lag was based on our understanding of how incumbent governments like to time the packages; to receive credit for taking such an action, but to escape punishment for being overtly political. In the interest of robustness, however, we ran the model with this variable at different lags. The results were statistically insignificant coefficients on this variable at time t (a lag that predicted failure perfectly), time t-1 (coefficient = 0.832 and significance = 0.352), and time t-3 (coefficient = 0.325 and significance = 0.775).

[49] As in the case of national elections, our determination that a one-month lag on the "new government" variable was appropriate was theoretically based. To determine if this was the only statistically significant lag, however, we ran the model with the "new government" variable at lags t(0), t-2, and t-3. In each case, the new government variable predicted failure perfectly.

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raises the odds of a package being announced by approximately 11.6 times.[50]

The coefficients for year-on-year changes in the nation's production index were all statistically insignificant, although the coefficient at time t-2 was just out of range ($p < .08$). Unfortunately, it was only at lags three and four that the signs on this macroeconomic conditions control variable were in the correct direction. Weak results also characterized the coefficients on our cabinet support variable. None of its coefficients were statistically significant, and the sign was in the wrong direction at lag five.[51] Our motivation for including cabinet support rates and year-on-year changes in the nation's production index was to be certain that our principal foreign and domestic politics variables were independently operative, as our theoretical discussion suggests. However, it is entirely possible that the inclusion of these variables was unnecessary and that a reduced model retains the same level of explanatory power. To test this idea, we must determine if the coefficients on our cabinet support and production index variables are zero.

Multiplying the difference between the models' log likelihoods by −2 produces a theoretical chi square of 7.53. At six degrees of freedom, we obtain $p < .30$ and therefore must accept the null hypothesis that coefficients for our two sets of control variables are zero, allowing us to use a reduced model. A comparison of the reduced model's estimates with those of the original model is presented in Table 3. The results indicate that the reduced model is well within the range of statistical significance and that coefficients on the foreign pressure and domestic politics variables are statistically significant at the same levels as in the original model. The one difference suggested by the new estimates is that the substantive impact of each of our explanatory variables is a little lower than in the reduced model. Regardless, the theoretical relationships specified earlier in the article have all been confirmed by our results—

[50] Because foreign pressure was not operative until after 1986, we ran the model with a structural break variable that produced no change in the overall fit of the model and no substantive or statistical change in its coefficients. Moreover, the log odds ratio for the structural break variable was small (0.402) and well out of the range of statistical significance ($p < .55$). When we ran the model for the post-1986 period, results were somewhat different. The overall statistical significance of the model did not change, but the substantive and statistical significance of some of the variables did move. In the post-1986 period elections were much more important than for the whole series (a log odds ratio of 2.718) while *gaiatsu* dropped but only very slightly (a log odds ratio of 2.219). The log odds ratio on the new government variable represented the most important change; although it remained virtually unchanged in size, it moved just out of the range of statistical significance.

[51] Running the model at different lags for cabinet support and year-on-year changes in the production index resulted in differences of neither substantive nor statistical significance for these two control variables.

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TABLE 3
POLITICS, PRESSURE, AND ECONOMIC STIMULUS POLICIES:
ORIGINAL AND REDUCED MODELS

	Original Model			*Reduced Model*		
Variables	*Estimates*	*SE*	*Z*	*Estimates*	*SE*	*Z*
Constant	−3.633**	0.472	−7.706	−3.525**	0.360	−9.488
Elections (t-2)	1.845*	0.861	2.143	1.689*	0.770	2.193
Gaiatsu						
U.S. pressure	2.515**	0.842	2.981	2.147**	0.768	2.799
New govt. (t-1)	1.712*	0.790	2.168	1.535*	0.742	2.069
	Total cases 274			Total cases 278		
	Log likelihood −47.728			Log likelihood −51.577		
	Chi square 20.86			Chi square 13.61		
	DF = 265; Significance = 0.0012			DF = 274; Significance = 0.0035		

** P < .01; * P < .05.

Japanese economic stimulus policies during the last three decades have been influenced by domestic politics and foreign pressure. As previously discussed, we wanted to be sure that we addressed the potential endogeneity problem with respect to our elections variable; thus we estimated the model using an elections variable that excluded elections held more than six months before constitutionally required and it produced virtually no change in our results. Log odds ratios on *gaiatsu* and new governments were the same—both substantively and statistically—while the log odds ratio on the elections variable increased slightly from 1.689 to 1.771 in the model with the amended variable.[52]

Having shown that domestic politics and foreign pressure are the principal factors behind the Japanese government's use of economic stimulus policies over the last twenty-five years, we conclude analysis with a brief statement of why these findings are important. A thorough analysis of the impact of Japan's economic stimulus policies is beyond the scope of this article, but we contend that the Japanese government's use of these policies in response to foreign and domestic pressure has been attendant to fiscal implications. As previously stated, supplementary budgets were passed by the government almost every year for a variety of reasons, including funding economic stimulus packages. For the period covered by our series, supplementary budgets were, on average, 3.25 percent of the Japanese government's general account budget. In

[52] This proved to be a statistically insignificant difference.

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the year following the announcement of an economic stimulus package, however, this percentage increased to 4.05 percent, while in other years, it was 2.72 percent. In view of this, we believe the government's use of economic stimulus policies did have an impact on the budgeting process in Japan, but a detailed investigation will have to be the subject of additional research.

Appendix

National Elections

UPPER HOUSE	LOWER HOUSE
July 7, 1977	December 12, 1976
June 6, 1980	October 10, 1979
June 6, 1983	June 6, 1980
July 7, 1986	December 12, 1983
July 7, 1989	July 7, 1986
July 7, 1992	February 2, 1990
July 7, 1995	July 7, 1993
July 12, 1998	October 10, 1996

Cabinet Changes That Resulted in the Formation of New Governments

Formation of the first Fukuda cabinet, December 1976
Formation of the first Ohira cabinet, December 1978
Formation of the first Suzuki cabinet, July 1980
Formation of the first Nakasone cabinet, November 1982
Formation of the first Takeshita cabinet, November 1987
Formation of the first Kaifu cabinet, August 1989
Formation of the first Miyazawa cabinet, November 1991
Formation of the first non-LDP (coalition) government since fall 1955 led by Japan New Party president, Morihiro Hosokawa, August 1993
Formation of minority (coalition) non-LDP government under New Life Party leader, Hata Tsutomu, April 1994
Formation of first Socialist-led government since 1947 under Murayama Tomoichi, June 1994
Formation of LDP-led government under Hashimoto Ryutaro, January 1996, after Murayama announces his resignation

Gaiatsu: Bilateral Meetings between U.S. Presidents and Japanese Prime Ministers (Includes Only Meetings Coded as *Gaiatsu*)

October 1986: Nakasone-Reagan
April 1987: Nakasone-Reagan
February 1989: Takeshita-Bush

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September 1989: Kaifu-Bush
March 1990: Kaifu-Bush
July 1991: Kaifu-Bush
January 1992: Miyazawa-Bush
April 1993: Miyazawa-Clinton
February 1994: Hosokawa-Clinton
January 1995: Murayama-Clinton
April 1996: Hashimoto-Clinton
April 1997: Hashimoto-Clinton

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THE BENEFITS OF ETHNIC WAR
Understanding Eurasia's Unrecognized States

By CHARLES KING*

WAR is the engine of state building, but it is also good for business. Historically, the three have often amounted to the same thing. The consolidation of national states in western Europe was in part a function of the interests of royal leaders in securing sufficient revenue for war making. In turn, costly military engagements were highly profitable enterprises for the suppliers of men, ships, and weaponry. The great affairs of statecraft, says Shakespeare's Richard II as he seizes his uncle's fortune to finance a war, "do ask some charge." The distinction between freebooter and founding father, privateer and president, has often been far murkier in fact than national mythmaking normally allows.

Only recently, however, have these insights figured in discussions of contemporary ethnic conflict and civil war. Focused studies of the mechanics of warfare, particularly in cases such as Sudan, Liberia, and Sierra Leone, have highlighted the complex economic incentives that can push violence forward, as well as the ways in which the easy labels that analysts use to identify such conflicts—as "ethnic" or "religious," say—always cloud more than they clarify.[1] Yet how precisely does the chaos of war become transformed into networks of profit, and how in turn can these informal networks harden into the institutions of states? Post-Soviet Eurasia provides an enlightening instance of these processes in train.

In the 1990s a half dozen small wars raged across the region, a series of armed conflicts that future historians might term collectively the

* The author would like to thank three anonymous referees for comments on an earlier draft of this article and Lori Khatchadourian, Nelson Kasfir, Christianne Hardy Wohlforth, Chester Crocker, and Michael Brown for helpful conversations.

[1] See David Keen, *The Benefits of Famine: A Political Economy of Famine Relief in Southwestern Sudan, 1983–1989* (Princeton: Princeton University Press, 1994); William Reno, *Corruption and State Politics in Sierra Leone* (Cambridge: Cambridge University Press, 1995); Stephen Ellis, *The Mask of Anarchy: The Roots of Liberia's War* (New York: New York University Press, 1999).

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wars of the Soviet succession: Nagorno-Karabakh, Transnistria, South Ossetia, Abkhazia, Chechnya, Tajikistan. Each involved a range of players, including the central governments of newly sovereign states, territorial separatists, the armed forces of other countries, and international peacemakers. By the middle of the decade, most of the conflicts had decrescendoed into relative stability. Numerous rounds of peace negotiations were held under the aegis of the United Nations and the Organization for Security and Cooperation in Europe (OSCE). Little progress was achieved in the talks, but with the exception of the second Chechen war beginning in 1998, none of the post-Soviet disputes returned to the previous levels of organized violence.

But how can one explain the persistence of these disputes, sometimes referred to as "stalled" or "frozen" conflicts, even after the cessation of violence? This article makes two central arguments in this regard. First, the territorial separatists of the early 1990s have become the state builders of the early 2000s, creating de facto countries whose ability to field armed forces, control their own territory, educate their children, and maintain local economies is about as well developed as that of the recognized states of which they are still notionally a part. The crystallization of independent statelike entities has meant that the resolution of these conflicts is not so much about patching together a torn country as about trying to reintegrate two functionally distinct administrations, militaries, and societies.[2] The products of the wars of the Soviet succession are not frozen conflicts but are, rather, relatively successful examples of making states by making war.

Second, the disputes have evolved from armed engagements to something close to equilibrium. In many cases both the separatists and their erstwhile opponents in central governments benefit from the untaxed trade and production flowing through the former war zones. Even in less unsavory ways, individuals inside and outside the conflict areas have an interest in maintaining the status quo—from poets who have built careers extolling their newfound statehood to pensioners worried about how their meager incomes might be further diminished if the country were once again integrated. It is a dark version of Pareto efficiency: the general welfare cannot be improved—by reaching a genuine peace accord allowing for real reintegration—without at the same

[2] By "statelike entity," I mean a political unit that has (1) a population and (2) a government exercising sovereign control over some piece of territory—but without the imprimatur of international recognition. In Eurasia the conceptual bar for statehood cannot be raised too high, for many of the qualities that define relatively well functioning states in central Europe do not exist farther east, even among "states" that have seats at the United Nations.

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time making key interest groups in both camps worse off. Even if a settlement is reached, it is unlikely to do more than recognize this basic logic and its attendant benefits.

This article examines the ways in which statelike entities have emerged and thrived in Eurasia since the earliest outbreak of violence in the late 1980s. Section I offers a brief overview of current research on civil war endings and notes the disjuncture between approaches drawn from the international relations literature and the work of sociologists and development economists on the functions of violence. Section II outlines the course of four Eurasian wars and identifies the de facto states that have arisen in their aftermath: the republic of Nagorno-Karabakh (in Azerbaijan); the Dnestr Moldovan republic, or Transnistria (in Moldova); and the republics of Abkhazia and South Ossetia (in Georgia).[3] There are other areas across eastern Europe and Eurasia that might be included on this list, such as Montenegro and Kosovo in Yugoslavia and Chechnya in the Russian Federation, not to mention the long-lived Turkish Republic of Northern Cyprus. But the four cases examined here are instances in which local armed forces, often with substantial assistance from outside powers, effectively defeated the armies of recognized governments in open warfare. They are also the cases in which the drive to create independent state structures has raised the most serious questions about whether one can reasonably expect any real reintegration with the central governments that are now recognized as legitimate by the international community. Section III analyzes the pillars of state building in each case, including the ways in which the interests of several major groups are satisfied by the limbo status into which these disputes have lapsed. Section IV describes the equilibrium that the disputes seem to have reached and suggests lessons that the cases might hold for further study of intrastate violence.

I. Civil War, Negotiations, and State Construction

Scholars have long recognized that civil wars tend to be protracted and that negotiated settlements are rare; even where talks succeed, they tend to produce end states that are less stable than outright victory by one

[3] In deeply divided societies even spelling bees are political events, so place-names in each of these instances are controversial. I use the Romanian Transnistria instead of *Pridnestrov'e* or *Transdniestria* because it is more easily pronounceable, and Abkhazia, South Ossetia, and Karabakh because few people will have heard of alternative designations such as *Apsny*, *Iryston*, and *Azat Artsakh*. The same rule of convenience applies to other proper nouns.

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side.[4] Given these facts—and the apparent interest of the international community in promoting negotiations, nonetheless—understanding why some belligerents come to the bargaining table while others remain on the battlefield has been a question of central importance.

Researchers have pointed to two broad categories of explanations. In one view, the qualities of the belligerents themselves may work against compromise. Ethnic groups may feel that a particular piece of real estate is historically theirs and that allowing it to be controlled by an alien group would be tantamount to national betrayal.[5] Committed leaders may sense that they have little choice but to push forward with the fight, lest they fall victim to even more radical comrades in their own camp.[6] If groups feel that they can get more by fighting than by negotiating—if they have not reached a "hurting stalemate," in William Zartman's well-known phrase—they are unlikely to seek peace.[7] A second view stresses the structural environment in which decision making takes place. Using insights from neorealist theory, some writers have argued that, in the absence of institutions to ensure credible commitment, even the most well intentioned leaders would be irrational to seek a negotiated settlement.[8] Given the host of factors that seem to work against negotiations, other observers have held that seeking peace only after one side has won or accepting the physical separation of warring ethnic groups may be the only truly stable solutions to large-scale communal violence.[9]

In all of these debates, however, the benefits of war have been largely neglected. As David Keen has observed, a major breakthrough in med-

[4] Paul R. Pillar, *Negotiating Peace: War Termination as a Bargaining Process* (Princeton: Princeton University Press, 1983), 25; Stephen John Stedman, *Peacemaking in Civil War: International Mediation in Zimbabwe, 1974–1980* (Boulder, Colo.: Lynne Rienner, 1991), 9; Roy Licklider, "The Consequences of Negotiated Settlements in Civil Wars, 1945–1993," *American Political Science Review* 89 (September 1995), 686.

[5] On ethnic war, see Chaim Kaufmann, "Possible and Impossible Solutions to Ethnic Civil Wars," *International Security* 20 (Spring 1996).

[6] Rui J. de Figueiredo, Jr., and Barry R. Weingast, "The Rationality of Fear: Political Opportunism and Ethnic Conflict," in Barbara F. Walter and Jack Snyder, eds., *Civil Wars, Insecurity, and Intervention* (New York: Columbia University Press, 1999).

[7] I. William Zartman, *Ripe for Resolution: Conflict and Intervention in Africa* (Oxford: Oxford University Press, 1985).

[8] Barry R. Posen, "The Security Dilemma and Ethnic Conflict," in Michael E. Brown, ed., *Ethnic Conflict and International Security* (Princeton: Princeton University Press, 1993); James D. Fearon, "Commitment Problems and the Spread of Ethnic Conflict," in David A. Lake and Donald Rothchild, eds., *The International Spread of Ethnic Conflict: Fear, Diffusion, and Escalation* (Princeton: Princeton University Press, 1998); Barbara F. Walter, "The Critical Barrier to Civil War Settlement," *International Organization* 51 (Summer 1997).

[9] Chaim Kaufmann, "When All Else Fails: Ethnic Population Transfers and Partitions in the Twentieth Century," *International Security* 23 (Fall 1998); Edward N. Luttwak, "Give War a Chance," *Foreign Affairs* 78 (July–August 1999).

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icine was the realization that what might be very bad for the organism could be very good for the germ that attacked it; the same can be said for civil wars.[10] There is a political economy to warfare that produces positive externalities for its perpetrators. Seemingly perpetual violence in Sierra Leone, Myanmar, Liberia, and elsewhere has less to do with anarchy—of either the social or the institutional kind—than with the rational calculations of elites about the use of violence as a tool for extracting and redistributing resources. Diamonds in Angola, timber in Cambodia, and coca in Colombia have all become spoils of war that fuel conflict while discouraging settlement. Conflicts, in this sense, may not "burn themselves out," precisely because it is in the interests of their makers, on all sides, to stoke them.[11]

Even after one camp has secured a partial or complete victory in the military contest, the basic networks, relationships, and informal channels that arose during the course of the violence can replicate themselves in new, statelike institutions in the former conflict zones. Belligerents are often able to craft a sophisticated array of formal institutions that function as effective quasi states, from the Jaffna Peninsula in Sri Lanka, to the "Somaliland republic" in Somalia, to the demilitarized zone in south-central Colombia. Through these institutions, however, politics in peacetime becomes little more than an extension of war. In the long run the instruments of violence, sublimated into the institutions of unrecognized regimes, keep existing states weak, populations poor, and full-scale war a constant possibility, even as they enrich the key players who extol the virtues of peace.

Such has been the case in the Eurasian conflicts of the 1990s. Yet there is also an intriguing twist. Not only have erstwhile separatists become relatively successful state builders, but they have also sometimes done so with the collusion of central governments, external actors, and international negotiators ostensibly committed to re-creating a stable, reintegrated country.

[10] Keen, "When War Itself Is Privatized," *Times Literary Supplement*, December 29, 1995. For a full exposition of his argument, see idem, *The Economic Functions of Violence in Civil Wars*, Adelphi Paper 320 (Oxford: Oxford University Press and International Institute for Strategic Studies, 1998).

[11] These arguments, central to the study of conflicts in the developing world for some time, have only recently begun to filter into the study of regional and interethnic violence in other areas. Even more recent is the attempt to see the uses of substate war through a broad, comparative lens. Among the most important works in this field are William Reno, *Warlord Politics and African States* (Boulder, Colo.: Lynne Rienner, 1998); Mats Berdal and David M. Malone, eds., *Greed and Grievance: Economic Agendas in Civil Wars* (Boulder, Colo.: Lynne Rienner, 2000); *A Rough Trade: The Role of Companies and Governments in the Angola Conflict* (London: Global Witness, 1998); and several working papers by Paul Collier and his associates at the Development Research Group of the World Bank, e.g., Collier, "On the Economic Consequences of Civil War," *Oxford Economic Papers* 51 (January 1999).

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II. The Wars of the Soviet Succession

The end of Soviet communism was a relatively peaceful affair. Notwithstanding the range of social grievances and disputed boundaries across the region, few of the rivalries actually produced open war.[12] But in at least four instances, interethnic disputes, external interests, and elite rivalries interacted to create wars that led to serious loss of life and resulted in hundreds of thousands of refugees and internally displaced persons (IDPs). In all four cases separatists actually won the armed conflicts, producing recognized states that are only marginally functional and unrecognized ones whose ability to govern themselves is surprisingly strong.

The dispute over Nagorno-Karabakh was not the first instance of open interethnic rivalry within Mikhail Gorbachev's Soviet Union, but it was the first that involved the interests of two Soviet republics, Armenia and Azerbaijan. Although included within the administrative boundaries of the Azerbaijan Soviet republic, Nagorno-Karabakh was populated in the main by ethnic Armenians, around 80 percent of the population by 1989. The region had enjoyed autonomous status since the very beginning of the Soviet Union, but Karabakh Armenians complained of cultural discrimination and economic underdevelopment.[13] With the increasing openness under Gorbachev, these issues came to the fore. In 1988 Karabakh leaders called for transferring the region to Armenian jurisdiction. Swift reprisals followed, including an organized pogrom against Armenians in the city of Sumgait in Azerbaijan. Both sides voiced profound grievances. From the Armenian perspective, repeated attacks on ethnic Armenian communities were reminiscent of the Ottoman-era genocide, especially given the massive outflow of over 180,000 refugees by mid-1989.[14] From the Azeri perspective, Armenians were attempting to squelch the Azeri national movement by destroying the republic's integrity.

In 1989 the Armenian Supreme Soviet and the Karabakh local council adopted a joint resolution declaring the unification of Armenia and Karabakh, and Armenia began supplying local paramilitary groups with substantial assistance in men and matériel. The Azeris responded

[12] James D. Fearon and David D. Laitin, "Explaining Interethnic Cooperation," *American Political Science Review* 90 (December 1996).

[13] For an enlightening overview of the origins and course of the Karabakh war, see David D. Laitin and Ronald Grigor Suny, "Armenia and Azerbaijan: Thinking a Way Out of Karabakh," *Middle East Policy* 7 (October 1999).

[14] V. A. Zolotarev, ed., *Rossiia (SSSR) v lokal'nykh voinakh i voennykh konfliktakh vtoroi poloviny XX veka* (Russia [USSR] in local wars and armed conflicts in the second half of the twentieth century) (Moscow: Institute of Military History, Russian Ministry of Defense, 2000), 45.

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TABLE 1
EURASIA'S RECOGNIZED AND DE FACTO STATES[a]

	Capital	*Independence and Recognition*	*Population*	*Ethnic Composition*	*Territory*	*Armed Forces*
Azerbaijan	Baku	declared Oct. 18, 1991; joined UN Mar. 9, 1992	8,000,000	Azeris 90%; Dagestani ethnic groups 3%;Russians 3%; Armenians 2%	86,600 sq. km.	72,100
Republic of Nagorno-Karabakh (also known as Azat Artsakh in Armenian)	Stepanakert	declared Sept. 2, 1991	150,000	Armenians 95%; Kurds, Greeks, Assyrians 5%	4,400 sq. km.	15,000–20,000 (incl. 8,000 from Armenia)
Moldova	Chisinau	declared Aug. 27, 1991; joined UN Mar. 2, 1992	4,300,000	Moldovans 65%; Ukrainians 14%; Russians 13%; Gagauz 4%	33,700 sq. km.	9,500
Dnestr Moldovan Republic (also known as Pridnestrov'e in Russian and Transnistria in Romanian)	Tiraspol	declared Sept. 2, 1990	670,000	Moldovans 33%; Russians 29%; Ukrainians 29%	4,163 sq. km.	5,000–10,000
Georgia	Tbilisi	declared Apr. 9, 1991; joined UN July 31, 1992	5,500,000	Georgians 70%; Armenians 8%; Russians 6%; Azeris 6%; Ossetians 3%; Abkhaz 2%	69,700 sq. km.	26,900

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TABLE 1 *(cont.)*

Republic of Abkhazia (also known as Apsny in Abkhaz)	Sukhumi	declared Aug. 25, 1990	200,000	mainly Abkhaz, but compact Armenian population in north and Georgians (Mingrelians) in south	7,867 sq. km.	5,000
Republic of South Ossetia (also known as Iryston in Ossetian)	Tskhinvali	declared Sept. 20, 1990; recognized by North Ossetia in 1993	70,000–80,000	mainly Ossetians, but some Russians and Georgians	2,732 sq. km., minus a few villages still under central government control	2,000

SOURCES: *The Military Balance, 2000–2001* (London: International Institute for Strategic Studies, 2000); K. G. Dzugaev, ed., *Iuzhnaia Osetiia: 10 let respublike* (South Ossetia: On the tenth anniversary of the republic) (Vladikavkaz: Iryston, 2000); *Sakartvelo/Georgia* (Tbilisi: Military Cartographic Factory, 1997); *Atlas of the Dniester Moldavian Republic* (Tiraspol: Dnestr State Cooperative University, 1997); www.worldbank.org; author interviews.

[a] Figures for the unrecognized states are, at best, imperfect estimates, but they are as close as one can come given the available evidence. Most unrecognized states declared sovereignty first within the context of the Soviet Union, then declared full independence; the first date is the one usually celebrated as the national holiday. Territory and population figures for recognized states also include the unrecognized republics. Military figures do not include reserves, which can quintuple the number of men under arms.

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by forcibly evacuating villages along the Armenian-Karabakh border and imposing a road and rail blockade first on the province and eventually on Armenia as well. Hostilities escalated after the collapse of the Soviet Union. Local Armenians in the regional capital, Stepanakert, organized a referendum on independence and declared the creation of a fully separate Republic of Nagorno-Karabakh. By the middle of 1992 Karabakh forces had opened a land corridor linking Karabakh to Armenia and had driven the Azerbaijani army from Shushi, the last remaining stronghold within Karabakh and a strategic highland from which the military had been able to bombard Stepanakert. A major offensive in 1993 created an Armenian-controlled buffer zone around Karabakh. After several unsuccessful mediation attempts throughout the early 1990s, the Russian Federation finally managed to secure a cease-fire in May 1994. Although it has since remained in place, with some minor violations, little real progress has been made on deciding Karabakh's final status.

The dispute between Armenians and Azeris might be cast, simplistically, as a reprise of struggles between Armenians and Turks left over from the early twentieth century. But across the Black Sea, in Moldova, no one would have predicted major violence in the 1980s. Rates of ethnic intermarriage were high; there were no religious lines separating ethnic minorities from the majority; and there had been no history of widespread communal violence. Nevertheless, Moldova became embroiled in a small war in the eastern part of the country, the thin Transnistria region east of the Dnestr River on the border with Ukraine.

Transnistrians were not a distinct ethnic population; in fact, ethnic Moldovans were the largest single group in the region. However, the importance of the zone in Soviet steel production and the military sector meant that Transnistria's inhabitants were fundamentally linked—in terms of both livelihood and social identity—to Soviet institutions such as the Communist Party, strategic industries, and the military.[15] The Moldovan national movement of the late 1980s thus hit Transnistrians particularly hard. Prodemocracy groups saw in perestroika an opportunity to reassert the voice of the republic's ethnic Moldovan majority after decades of Russian cultural domination. In 1989 the republican Supreme Soviet adopted a series of language laws that made Moldovan (Romanian) the state language and mandated the use of the Latin alphabet instead of Cyrillic.

[15] See Charles King, *The Moldovans: Romania, Russia, and the Politics of Culture* (Stanford, Calif.: Hoover Institution Press, 2000), chap. 9.

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Industrial managers and military personnel in Transnistria reacted sharply, taking control of governmental and security structures in the districts east of the Dnestr River and in the Russian-majority city of Bender on the west bank. In autumn 1990 Transnistrian leaders declared a separate republic within the Soviet Union and later opted for full independence when Moldova itself seceded from the Soviet federation. War accompanied these competing declarations. In 1992 a Moldovan government offensive against Bender sparked the first major intervention by the Russian Fourteenth Army, stationed in Transnistria, on the side of the separatists. With the superior firepower of the Russian troops, the Moldovans were driven out of the city. The uneasy balance of power after the battle produced a formal cease-fire agreement and the deployment of a tripartite Russian-Moldovan-Transnistrian peacekeeping force. Despite numerous rounds of talks, sponsored by the OSCE and regional neighbors, there is as yet no agreement on the final status of Transnistria.

On the surface the relationship between Georgians and Abkhaz had little in common with that between Moldovans and Transnistrians. The Abkhaz are a distinct ethnic population, speaking a language unrelated to Georgian. During the Soviet period the Abkhaz were given their own autonomous republic, within which they enjoyed a privileged position in the party and state hierarchy, even though they constituted less than one-fifth of the population there. However, the pattern of events in the late 1980s paralleled those in Moldova. A revitalized Georgian national movement emerged in the waning days of Soviet power, eventually leading to a referendum on independence and Georgia's secession from the Soviet Union.

Abkhaz reacted by demanding greater local autonomy and a say in the politics of independent Georgia. Clashes erupted between the Abkhaz and the local Georgian majority. In early 1992 a new Georgian president, Eduard Shevardnadze, repudiated the negotiations that had been ongoing with the Abkhaz leadership, and full-scale war followed. Georgian troops marched into Abkhazia in an effort to eject the regional government and succeeded in capturing and holding the regional capital, Sukhumi. But by the end of 1993 Abkhaz militias, assisted by Russian forces, had pushed back the ill-prepared Georgian troops to the Inguri River, the dividing line between Georgia proper and Abkhazia. A Russian-brokered agreement in May 1994 provided for the deployment of a peacekeeping mission of the Commonwealth of Independent States (CIS) (in practice wholly Russian) to monitor the

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security zone along the river. Negotiations on Abkhazia's final status, brokered by the United Nations, have continued since then.

Unlike the Abkhaz, the Ossetians had not historically been concentrated in Georgia, in the area of present South Ossetia; their cultural center was across the border in North Ossetia, now part of the Russian Federation.[16] By 1989, however, two-thirds of South Ossetia's population was ethnic Ossetian. Despite a history of strong intercultural ties between Georgians and Ossetians, the political climate of the late 1980s encouraged cascading demands for local autonomy and independence. In 1988 and 1989 the Georgian government adopted measures to increase the use of the Georgian language in public life and shortly thereafter rejected demands by regional leaders to upgrade South Ossetia's status from "autonomous region" to autonomous republic, the same as Abkhazia's. As with the Transnistrian reaction against Moldovan language reforms, Ossetian leaders also argued that language reforms would unfairly disadvantage them. The spark that ignited the violence, however, came in 1990, when the regional administration declared a separate South Ossetian republic within the Soviet Union, moved to unite with North Ossetia, and shortly thereafter held elections for a separate South Ossetian parliament—a variation on the Karabakh theme. In response, the Georgian parliament voted to revoke South Ossetia's existing autonomous status. President Shevardnadze ordered troops to the region, but their entry met with fierce resistance from Ossetian irregulars and their supporters from North Ossetia and other parts of the Russian Federation. In July 1992 a cease-fire agreement provided for the cessation of hostilities and final-status negotiations under the auspices of the OSCE.

III. The Politics of Surreptitious State Building

The political elites that made these wars are today in large part the same, both in the national capitals and in the separatist regions. Most continue to refer to the events of the late perestroika period as explanations for why the violence erupted and why a stable settlement has been so elusive. Karabakh leaders talk of the revocation of their local autonomy and the massacre of ethnic Armenians in Sumgait. Transnistrians speak of the threat of cultural "romanianization" and the unwelcome possibility of Moldovan unification with Romania. Abkhaz and Osse-

[16] For a firm statement of this view, see Anzor Totadze, *The Ossets in Georgia* (Tbilisi: Samshoblo, 1994); Totadze is the Georgian deputy minister of labor, health, and social affairs.

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tians list Georgia's oppressive cultural policies and the dilution of the local autonomy that both regions had during the Soviet years.

These putative root causes, however, are slippery explanations for the absence of a final settlement. Most central governments and international organizations have in fact done everything that the conventional wisdom on conflict resolution would suggest in order to reach an equitable solution. Generally stable cease-fires, monitored by outside parties, have been put in place. Regular negotiations have continued under the aegis of the UN and the OSCE, with the support of the United States and the Russian Federation. Governments have, to varying degrees, amended their constitutions, citizenship laws, educational statutes, and local administrative structures to provide for civil rights guarantees and local autonomy, all of which has allowed all three recognized states—Azerbaijan, Moldova, and Georgia—to join the Council of Europe.

The real obstacle to a final settlement has been the fact that, beneath the façade of unresolved grievances and international negotiations, political elites in each region have managed to build states that now function about as well as the recognized countries of which they are still formally constituents. These unrecognized entities, moreover, are shielded by independent militaries, all of which have substantial supplies of armor and equipment: 15,000 to 20,000 men in Karabakh, 5,000 to 10,000 in Transnistria, 2,000 in South Ossetia, 5,000 in Abkhazia.[17] At the same time interest groups outside the conflict zones have learned to live with the effective division of their countries, finding ways to profit from a state apparatus that is chronically weak—and, in the process, ensuring that it remains so. The mechanisms of surreptitious state building have become increasingly clear in each case: the economic benefits of state weakness, the support of key external actors, the legitimization of statehood through cultural and educational policies, the complicity of central governments, and in some instances the unwitting assistance of international negotiators.

The Political Economy of Weak States

By any measure, Azerbaijan, Georgia, and Moldova are exceptionally weak states. Per capita GNP in 1999 was under $650 in all three countries. In the first two, public revenues (including foreign grants) account for 20 percent or less of GDP, a figure too low to support even the most

[17] *The Military Balance, 2000–2001* (London: International Institute for Strategic Studies, 2000), 100.

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basic state functions.[18] Significant portions of each country's territory, population, and wealth-producing potential—the separatist regions—remain wholly outside central government control. Karabakh and the occupied buffer areas are about 20 percent of Azerbaijan's territory; Abkhazia and South Ossetia together are 17 percent of Georgia's; Transnistria is 12 percent of Moldova's. Even outside the separatist republics, there are many parts of the country where the central government's power is virtually nil, areas where banditry is common, local notables run their own affairs, and the institutions of the central state are conspicuous by their absence. The lives of average Azerbaijanis, Georgians, and Moldovans rarely intersect with the state, and where they do, it is often in the form of a policeman demanding payment for an imagined traffic offense.

State weakness is of obvious benefit to the unrecognized regimes. Business can be carried on with neighboring states without paying production taxes or tariffs. Luxury goods, especially cigarettes and alcohol, can be brought in for resale or export. The republics differ, though, in terms of their relative economic success. The lowest on the development scale is probably Karabakh. Situated in a mountainous area where most roads are barely passable and with little indigenous industry and a collapsed agricultural system, Karabakh is largely poverty stricken. Its total population, estimated at 150,000, survives mainly on the basis of subsistence farming or resale of goods imported from Iran and Armenia.[19] Important urban centers, such as the city of Shushi, have yet to rebuild apartment buildings and offices gutted during the war. Although demining of fields and villages has progressed with the assistance of international relief agencies since the cease-fire, agricultural production has remained stunted because of fear of unexploded ordnance.[20] Nevertheless, local authorities have been able to construct something resembling a state, with its own foreign ministry (which charges visitors $25 for visas), armed forces, police, and court system. Even in Karabakh's dire straits, citizens have been able to find economic potential. The export of wood to Armenia and farther afield has become a booming enterprise, but it has also caused serious worries about deforestation and the long-term effects on Karabakh's eroding agricultural land, a situation that also obtains in Abkhazia.[21]

[18] Economic figures are based on World Bank reports at www.worldbank.org.

[19] Author interviews in Stepanakert, September 27–28, 2000.

[20] Author interview with Edgar Sargsian, field officer, International Committee of the Red Cross, Stepanakert, September 28, 2000.

[21] Author interview with Tevfik Yaprak, World Bank head of mission, Tbilisi, October 11, 2000; *Svobodnaia Gruziia*, September 27, 2000, 4.

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Abkhazia and South Ossetia are only marginally better off than Karabakh. During the Soviet period, by contrast, both had been reasonably important regions. Abkhazia supported a booming tourist trade along its Black Sea coast, as well as a substantial hazelnut industry. In South Ossetia lead and zinc mines and factories producing enamel fittings, wood products, and beer and fruit juices had been important parts of the Georgian economy. Now, however, few of these enterprises are still functioning, since the outflow of refugees and IDPs more than halved the populations of both regions, which stand at under 200,000 in Abkhazia and 70,000 to 80,000 in South Ossetia.

Local inhabitants have turned to other pursuits. In Abkhazia tangerines and hazelnuts remain an important source of revenue, particularly since there are no taxes to pay to the central Georgian government; local gang activity, in fact, tends to be seasonal, centered around the attempts by bandits to steal hazelnut shipments in the late summer and early autumn. Trade in scrap metal, both from dysfunctional industries as well as from power lines, is also important. South Ossetia has little in the way of functioning industry or export-oriented agriculture, but the region's geographical position has been its chief asset. Just outside the entrance to the regional capital, Tskhinvali, the South Ossetian highway police maintain a customs checkpoint to monitor the vigorous trade along the highway to Vladikavkaz, the capital of the Russian republic of North Ossetia. The police, however, have come to function more as facilitators of this commerce than as its invigilators. A massive market in petrol and wheat flour flourishes along the roadside, with hundreds of trucks laden with goods from the Russian Federation.[22] The South Ossetian administration derives major amounts of revenue from controlling this trade, the road link to Vladikavkaz, and especially the passage through the mountain tunnel linking North and South Ossetia. OSCE officials estimate that some $60–$70 million in goods pass through the tunnel each year, compared with an official South Ossetian budget of roughly $1 million.[23] In both Abkhazia and South Ossetia, drugs, especially heroin, have also joined the list of transit goods.

Of all four unrecognized republics, Transnistria's economic position is probably the best. During the Soviet period Transnistria was the mainstay of Moldovan industry; while areas west of the Dnestr River were largely agricultural, most heavy machine industries and power-generating plants were located to the east.[24] Many still operate on the

[22] Author interviews in Tskhinvali, October 13, 2000.
[23] Author interview with Hans-Gjorg Heinrich, adviser to OSCE mission, Tbilisi, October 23, 2000.
[24] *IMF Economic Reviews: Moldova, 1993* (Washington, D.C.: International Monetary Fund, 1993), 46.

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basis of barter, but some have even managed to secure contracts with firms abroad. The Ribnita mill, in northern Transnistria, was one of the Soviet Union's most important producers of high-quality rolled steel, especially for munitions. Originally built in 1984 using German technology, the plant remains one of the best in the former Soviet Union, and firms from Western Europe continue to sign contracts with the plant—so many, in fact, that by the late 1990s the firm employed a bevy of translators to process foreign orders.[25] The plant's profits provide roughly half the revenue for Transnistria's state budget.[26] It is indicative of Transnistria's international links that the "Dnestr Moldovan republic ruble," introduced as the region's official currency in 1994, was printed in Germany. In addition to steel, small arms—an important local industry during the Soviet period—are also manufactured, and Transnistria's president, Igor Smirnov, has hailed their export as a sign of his republic's importance on the world stage and its links with other embattled peoples in Kosovo, Chechnya, Abkhazia, and elsewhere.[27] Given the dire state of Moldova's own economy, Transnistria looks rather better in some areas. Average household income is higher, and in every major field except consumer goods the separatist region is a net "exporter" to the rest of Moldova, delivering more construction materials, chemicals, ferrous metals, and electrical energy than it receives.[28]

Russia, Diaspora Politics, and Inter-"State" Cooperation

From the earliest days these conflicts were never simple confrontations between an embattled ethnic minority and a nationalizing central government. The relationships involved were even more complex than Rogers Brubaker's "triadic nexus"—ethnic minority, central government, external homeland—would suggest.[29] Indeed, many interested players have been crucial in assisting the separatist republics not only in winning the wars but also in consolidating statehood afterward.

By far the most significant has been the Russian Federation. The Russian official history of the post-Soviet wars argues that Moscow has played a pacifying role in each of the conflicts.[30] It is clear, though, that

[25] Author interviews with Transnistrian steel workers, Ribnita, August 1, 1997.

[26] *Oxford Analytica East Europe Daily Brief*, January 11, 1999.

[27] Author interview with Valeriu Prudnicov, Moldovan police commissioner, Bender, August 1, 1997.

[28] *Republic of Moldova: Economic Review of the Transnistria Region, June 1998* (Washington, D.C.: World Bank, 1998), 27.

[29] See Brubaker, *Nationalism Reframed: Nationhood and the National Question in the New Europe* (Cambridge: Cambridge University Press, 1996), esp. chap. 3.

[30] See Zolotarev (fn. 14), esp. chap. 8.

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Russian assistance was a crucial component in the early stages of state building. Whether prompted by the whim of brigade commanders or by a policy directive from Moscow, Soviet armed forces, later to become Russian Federation troops, were the main supplier of weaponry (and often soldiers) to separatist groups. Throughout 1991 and 1992 the Moldovans issued numerous notes to the Soviet and Russian governments protesting the involvement of the Soviet Fourteenth Army on the side of the Transnistrians.[31] In December 1991 the army's commander left his post to become head of the Dnestr Guards, the newly created army of the Dnestr Moldovan republic; he was followed by his former chief of staff, who became the republic's defense minister.[32] Azerbaijan was able to secure the complete withdrawal of Russian troops from its territory by mid-1993, but the forces that remained in Armenia—the Russian Seventh Army—are known to have aided both Armenian government troops and Karabakh irregulars during the war. Russian newspapers published the names of soldiers who participated in the fighting, and in 1992 the Russian Defense Ministry promoted the commanders of both the Fourteenth and Seventh Armies for their leadership in the Transnistrian and Karabakh campaigns.[33] Leakage of weapons and soldiers from the Russian 345th Airborne Regiment, based in Abkhazia, as well as the influx of freelance fighters from Russia's north Caucasus, contributed to the Abkhaz defeat of Georgian forces.[34]

Russian foreign and security policy since the wars has been complex in each of these cases, but it has centered around three main elements, all of which have turned out to be crucial resources for the unrecognized republics. First, Russian economic support has been essential. The Russian gas monopoly, Gazprom, while pressuring Azerbaijan, Moldova, and Georgia to pay their massive energy debt, has continued to supply subsidized gas to the separatist areas. Russian officials have even staffed positions within key economic institutions. Until late 1996 the head of the Transnistrian central bank was reportedly a member of

[31] *Moldova suverana*, June 11, 1991, 1; *Curierul national*, April 4, 1992, 1, 7; *Romania libera*, April 4–5, 1992, 8.

[32] Stephen Bowers, "The Crisis in Moldova," *Jane's Intelligence Review* (November 1992), 484.

[33] *Den'*, August 9–15, 1992, and Radio Maiak, September 18, 1992, both cited in Mihai Gribincea, *Politica rusa a bazelor militare: Moldova si Georgia* (Russian policy on military bases: Moldova and Georgia) (Chisinau: Civitas, 1999), 15. Gribincea's book is the most thorough study of the role of the Russian military in Moldova and Georgia. See also idem, *Trupele ruse in Republica Moldova: factor stabilizator sau sursa de pericol?* (Russian troops in the Republic of Moldova: Stabilizing factor or source of danger?) (Chisinau: Civitas, 1998).

[34] Gribincea (fn. 33, 1999), 42–43.

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the Russian intelligence service; even after that, bank officials continued to receive training in Moscow and St. Petersburg.[35]

Second, negotiations with Moldova and Georgia regarding the withdrawal of Russian troops have been linked to the resolution of the separatist disputes. In 1999 both Moldova and Georgia managed to secure Russian agreement to an eventual full-scale withdrawal, but in both cases the devil has been in the details. The Moldovan government, pressured by both Russia and the OSCE, signed an agreement in 1994 mandating that the withdrawal of the Fourteenth Army be "synchronized" with the final status of Transnistria. That agreement has effectively blocked real progress in withdrawal negotiations, since it is unclear whether withdrawal should precede resolution or vice versa. Russian troop strength is much lower now than in the past—in 2000, about twenty-six hundred men plus local contract hires, reorganized as an "operational group" rather than an army[36]—but the military presence continues to be a boon to the Transnistrians, providing civilian and military employment for local citizens and a sense of security for the unrecognized regime.[37]

The Russian military in Georgia began downsizing in 2000. However, much of the matériel was moved to Armenia, with which Russia has a long-term basing agreement; that, in turn, aroused Azerbaijani fears that some of the equipment would eventually find its way into both Armenian and Karabakh hands.[38] The Russian military base in Abkhazia serves much the same function as the troop presence in Transnistria, providing employment and security for an effectively separate regime. The Russian and Georgian governments have carried out negotiations regarding the conversion of the base into a convalescence station for Russian peacekeepers, but that change of label would not substantially alter the strong role that the facility plays in Abkhaz political and economic life.[39] In both Moldova and Georgia even the salaries of Russian soldiers and peacekeepers, paid in rubles, have en-

[35] Author interview with Elena Niculina, World Bank representative, Chisinau, July 29, 1997. The same point, however, could be made even about the recognized states. Russia continues to provide what amounts to subsidized gas deliveries, since outstanding debts from the former Soviet republics are often paid in government-issued bonds, which are, as Gazprom must realize, virtually worthless.

[36] *The Military Balance* (fn. 17), 125.

[37] A March 1995 referendum organized by the Transnistrian administration indicated that 93 percent of voters wanted a permanent Russian base in the region.

[38] Azerbaijani officials have suggested that the deployment of Turkish troops in Nakhichevan, the Azerbaijani enclave bordering Armenia, Turkey, and Iran, might be considered as a response to the increase in Russian forces in Armenia. *Svobodnaia Gruziia*, October 25, 2000, 4.

[39] *Georgia Today*, October 6–12, 2000, 4.

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sured that Transnistria, Abkhazia, and South Ossetia remain economically tied to Russia rather than to their recognized central governments, because local goods and services are purchased using rubles rather than national currencies.[40] For these reasons, the Transnistrians and the Abkhaz have insisted that the bases remain in place or, if they are closed, that the Russian military equipment be transferred to Transnistrian and Abkhaz control.[41]

Third, Russian citizenship and visa policy has encouraged the separatist regions to see themselves as effectively independent states. Azerbaijan, Moldova, and Georgia have all been wary of allowing dual citizenship, for fear that inhabitants of the unrecognized republics would secure foreign citizenship and become even further disconnected from the center.[42] Many have taken Russian citizenship nevertheless. According to the Transnistrian administration, as many as sixty-five thousand people (about 10 percent of the population) now hold Russian citizenship.[43] Georgian officials worry that Abkhaz and South Ossetians have done likewise, especially since much of their livelihood depends on the ability to travel easily to the Russian Federation. The citizenship option is another reason that contract work in Russian Federation forces in Abkhazia and Transnistria has been an attractive option for many locals, since it can lead to a passport and citizenship. Even for those who are not citizens, changes in Russian visa policy have also widened the gap between the separatist zones and the central governments. Under a previous visa regime, citizens of former Soviet republics could travel to Russia without a visa. But as part of a move to tighten border security in the wake of the Chechen wars, Russia announced that it would pull out of the agreement and begin requiring visas for citizens of certain post-Soviet states. From late 2000 regular Russian visas were required of citizens of Georgia—but not of inhabitants of South Ossetia and Abkhazia.

While overwhelmingly significant, Russia is not the only external dimension to state building. Diaspora politics has also played a role. Armenia and the Armenian diaspora have been the sine qua non of

[40] Russian peacekeeping forces, although under a separate command from regular army personnel, have had a similar influence on the local economy. By early 2000 there were around fifteen hundred Russian peacekeepers in Abkhazia (formally a CIS peacekeeping mission), five hundred in South Ossetia, and five hundred in Transnistria.

[41] *Svobodnaia Gruziia*, October 24, 2000, 3, and October 25, 2000, 1.

[42] In August 2000 Moldova adopted a new citizenship law that provides for dual citizenship based on bilateral agreements. Currently, however, Moldova does not have any such agreements with foreign countries.

[43] *Oxford Analytica East Europe Daily Brief*, June 29, 2000.

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Karabakh's existence. For all practical purposes, Karabakh is now more an autonomous district of Armenia than a part of Azerbaijan. The Armenian dram, not the Azerbaijani manat, is the legal tender. Substantial numbers of Karabakh inhabitants are Armenian citizens and travel abroad with Armenian passports; some have even risen to political office in Armenia—including Robert Kocharian, who has the distinction of having been president of both Karabakh and, now, Armenia. The highway connecting the Armenian city of Goris to Stepanakert, the so-called Lachin corridor carved out during the war, may now be the finest road in the entire south Caucasus. Built to European standards, it was financed in part by Armenians abroad, which accounts for the bizarre sign outside Stepanakert, in Spanish, acknowledging contributions from Argentina in its construction. Military convoys regularly travel the highway, taking fuel to Karabakh and returning to Armenia with timber, and there is nothing more than a small police checkpoint at the putative international frontier. Foreign investment from abroad, usually from Armenian communities, has begun to pick up. Swiss Armenian businessmen have invested some $900,000 in a watch-manufacturing facility; others have spent $2 million to renovate Stepanakert's central Hotel Karabakh; and still other investors have pledged some $17 million to build tourist facilities near Karabakh's striking medieval monasteries.[44]

The four unrecognized states also act in the international arena as if they were independent entities and cooperate with one another to a great degree. They have officially recognized each other's existence. The four presidents exchange visits during each republic's national day celebrations. Official delegations sign trade agreements, and firms execute import and export deals. Security services share information on possible threats. For example, in autumn 2000 a delegation of leaders of Moldovan nongovernmental organizations arrived in Georgia for a brief tour. The Moldovans asked, via the local OSCE office, if they could arrange a trip to South Ossetia as part of their program. After approaching the South Ossetian leadership, the OSCE brought back a categorically negative response. As it turned out, the deputy speaker of the Transnistrian parliament had been in South Ossetia only weeks earlier, to attend the celebrations surrounding the tenth anniversary of South

[44] Author interviews in Stepanakert, September 27–28, 2000; *Russia Journal*, October 7, 2000 (electronic version at www.russiajournal.com/weekly); *Radio Free Europe/Radio Liberty Armenia Report* (May 1, 2000). Diaspora support, however, has not been as enthusiastic as Karabakh leaders would like. A tour through the United States by Karabakh premier Anushavan Danielian in 2000 produced pledges of about $5 million. The campaign had hoped to raise four times that amount.

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Ossetian independence, and he had strongly advised the Ossetian interior and foreign ministries against approving the Moldovan visit.[45] Networks such as these were formalized in November 2000, when the foreign ministers of the four republics held an official conference in the Transnistrian capital, Tiraspol, and pledged to coordinate their bargaining positions in talks with the three central governments.

Making Denizens Into Citizens

From early in all four conflicts, local authorities moved to take over educational and cultural institutions within the conflict zones. Polytechnics were upgraded to universities, new "academies of science" were established, and new national festivals were inaugurated. History curricula were redesigned to present the citizens of the separatist regions as the indigenous inhabitants of their territory and to strengthen the connection between previous forms of statehood and the current, unrecognized states. The new ministry of information and press of the South Ossetian republic began to reproduce works of nineteenth-century travelers who described the customs of the Ossetians, in order "to bring to the masses the most interesting pages in the history of Ossetia and the Ossetians."[46] The Ossetians located the origins of their modern statehood in ancient Iryston, the lands of settlement of the Iranian-speaking Alans; they were thus considered, as a new Ossetian encyclopedia argued, the true "autochthonous population" in their republic.[47]

Local intellectuals also worked, as far as possible, to discover cultural or historical heroes around which semiofficial cults could be built. In Transnistria, Alexander Suvorov, the eighteenth-century field marshal who conquered Transnistria for the Russian Empire, became a symbol of the Dnestr Moldovan republic, his visage appearing on the newly minted Transnistrian ruble. In South Ossetia the statue of Kosta Khetagurov, a nineteenth-century poet, became one of the focal points of the annual "republic day" in September. Previous instances of statehood, however tenuous, were marshaled in the cause. Armenians in Karabakh pointed to their own briefly independent republic, which had existed before Karabakh's absorption into Soviet Azerbaijan in the 1920s. Abkhaz writers lauded their 1925 constitution, which estab-

[45] Author's interview with Igor Munteanu, director of the Viitorul Foundation, Tbilisi, October 12, 2000.

[46] Aleksandr Ianovskii, *Osetiia* (Ossetia) (Tskhinvali: Ministry of Information and Press of the Republic of South Ossetia, 1993), 3, from the editor's preface.

[47] A. B. Dzadziev, Kh. V. Dzutsev, and S. M. Karaev, *Etnografiia i mifologiia osetin* (Ethnography and mythology of the Ossetians) (Vladikavkaz: n. p., 1994), 64.

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lished an autonomous regime. Transnistrians identified the Moldovan autonomous republic, which had existed inside Soviet Ukraine between the two world wars, as the basis of their modern statehood.[48]

The armed conflicts themselves also came to be venerated as a struggle against external aggression. Children who were not even born when the conflicts began are now almost teenagers, schooled in the view that the republics they inhabit not only represent ancient nations but also have been forged in the crucible of war and sacrifice. A special textbook, published to celebrate South Ossetia's first decade of independence in 2000, proclaimed:

> Ten years ago . . . at the height of the Georgian-Ossetian confrontation, the Republic of South Ossetia was declared, a republic that has proved to be durable. The war killed and maimed thousands of our citizens; left tens of thousands of innocent people without shelter, work, and means of survival; razed our infrastructure; robbed the people of kindergartens and schools; and made peaceful citizens into refugees. Nevertheless, these years have a special historical significance for us, because we not only managed to defeat the aggressor but also to build our own statehood.[49]

Transnistrian textbooks proffer a similar narrative, especially with regard to the decisive Battle of Bender in 1992:

> The traitorous, barbaric, and unprovoked invasion of Bender had a single goal: to frighten and bring to their knees the inhabitants of the Dnestr republic. . . . However, the people's bravery, steadfastness, and love of liberty saved the Dnestr republic. The defense of Bender against the overwhelming forces of the enemy closed a heroic page in the history of our young republic. The best sons and daughters of the people sacrificed their lives for peace and liberty in our land.[50]

These arguments differ little from the equally tendentious views often used to justify the existence of Azerbaijan, Moldova, Georgia, and other new Eurasian states. As in those instances, there were rational reasons for the strategies that intellectuals and academics at the heart of these nation-building efforts pursued. In Karabakh the opportunity for greater connections with educational and research institutions in Armenia was at the center of the early movement for transferring the region to Armenian jurisdiction. Many Karabakh writers and educators

[48] Author interview with Vladimir Atamaniuk, first deputy speaker of the Supreme Soviet of the Dnestr Moldovan republic, Tiraspol, August 1, 1997.

[49] K. G. Dzugaev, ed., *Iuzhnaia Osetiia: 10 let respublike* (South Ossetia: On the tenth anniversary of the republic) (Vladikavkaz: Iryston, 2000), 4.

[50] N. V. Babilunga and V. G. Bomeshko, *Pagini din istoria plaiului natal* (Pages from the history of the fatherland) (Tiraspol: Transnistrian Institute of Continuing Education, 1997), 98.

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eventually moved to Russia or Armenia, but others found themselves catapulted into new jobs as professors and administrators of the new "Artsakh State University" in Stepanakert. In Moldova the purge of Soviet-era scholars in the late 1980s created a class of disgruntled researchers and writers who looked on the Transnistrian cause as their own. Although not native to the region, many moved to Transnistria, where they could continue to thrive by writing the same Soviet-style versions of history and socialist internationalism that had made their careers—and become the shapers of Transnistrian national identity in the process.[51] In South Ossetia professors at the local polytechnic found that increasing ties with institutions in Vladikavkaz, Moscow, and St. Petersburg was more appealing than continued existence as a backwater in an increasingly "georgianized" educational system. While the new ideologies of nationalism and statehood at times did violence to historical fact, most grew as much from the professional backgrounds and interests of their makers as from a romantic commitment to nationalist ideals.

The Complicity of Central Governments

Central authorities frequently point to the modalities of state building outlined above, complaining that the separatists and their external supporters are indeed constructing states that have come to depend less and less on the recognized governments. But that is only part of the story. In Georgia and Moldova central policy elites have also played a role in prolonging the disputes. The benefits of state weakness accrue not only to the separatists but also to the institutions and individuals who are ostensibly responsible for remedying it. Both countries are arguably among the most corrupt in the former Soviet Union, indeed, among the most corrupt in the world.[52] The links between corrupt central governments and the separatist regions have further imperiled already weak state structures while enriching those who claim to be looking after the states' interests.

In South Ossetia the illegal trade with Russia benefits all sides. The South Ossetian government receives money from resale and haphazardly applied "transit taxes," while Georgian authorities, especially the interior ministry, are able to take a cut by exacting fines from truck

[51] See, for example, N. Babilunga, ed., *Bessarabskii vopros i obrazovanie Pridnestrovskoi Moldavskoi Respubliki* (The Bessarabian question and the formation of the Dnestr Moldovan republic) (Tiraspol: Dnestr State Cooperative University, 1993); M. Shornikov, *Pokushenie na status* (Striving for status) (Chisinau: Chisinau Society of Russians, 1997).

[52] See the "Corruption Perceptions Index" at www.transparency.org.

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drivers on the outskirts of Tbilisi. The expansion of international humanitarian aid to the region has also provided another cover under which goods can be traded; organizations are set up in Tbilisi to receive assistance destined for South Ossetia, and the goods are then sold in local markets.[53] It is partly for these reasons that relations between Tskhinvali and Tbilisi have generally been cordial, notwithstanding the lack of a final settlement. The South Ossetian president, in fact, openly supported Eduard Shevardnadze in his campaign for Georgian president in early 2000.[54]

Similar formulas are applied in Abkhazia. Police officials in Zugdidi and Tsalenjikha, the two districts on the Georgian side of the border with Abkhazia, carry out periodic crackdowns on illegal transborder commerce, but local observers are convinced that these efforts are designed less to enforce the law than to root out small-time smugglers who might disrupt the police monopoly on transborder trade.[55] None of this is news to local Georgians, who express deep skepticism about their own state institutions: two-thirds report having no faith in parliament or the president, and some 80 percent have no faith in tax and customs officials.[56]

These connections are even easier to document in Transnistria. In accords signed in 1996 and 1997, the Moldovan government, encouraged by the OSCE, agreed to establish joint customs posts with the Transnistrian administration, providing official customs stamps and export licenses to the separatists. Transnistria was also given the right to import and export goods, directly or via other parts of Moldova, without paying duties at the entry to Moldovan-controlled territory. Although the agreement was intended as a measure to build confidence between the two sides, in practice it represents little more than a conduit for illegal commerce under the cover of law. The scale of this trade is easily traceable, since customs duties are duly registered with the Moldovan central government, even if the money never makes it into the state coffers. In 1998, for example, Moldova imported about $125 million in goods subject to import taxes. At the same time, another $500 million was registered with Moldovan customs officials as entering the country for transit on to Transnistria.[57]

[53] Author confidential interview with senior United Nations official, Tbilisi, October 30, 2000.

[54] *Radio Free Europe/Radio Liberty Caucasus Report* (April 7, 2000).

[55] Heinrich interview (fn. 23); author interview with Naira Gelashvili, director of Caucasus House, Tbilisi, October 3 and 23, 2000; *Ekho-Daidzhest*, August 1–15, 2000, 7.

[56] *Georgian Lifestyle Survey*, 2000, cited in *Human Development Report, Georgia 2000* (Tbilisi: United Nations Development Program, 2000), 74.

[57] *Buletinul social-democrat*, no. 2 (2000); and author conversations with Oazu Nantoi, Chisinau, September 2000.

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The figures are as instructive as they are incredible: a piece of territory that holds about 17 percent of Moldova's total population imported four times as much merchandise as the rest of the country, including about six thousand times as many cigarettes—all with the full knowledge of the central tax inspector's office. While some of the imports no doubt do reach Transnistria, most find their way on to the Moldovan market. The country's senior presidential adviser on Transnistria, Oazu Nantoi, resigned in protest when he discovered these figures, and he later organized a series of broadcasts on public television that brought this illegal trade to light. But in late 2000 the director of Moldovan National Television ordered the broadcasts stopped, reportedly on the order of senior government officials.[58] Throughout these conflict zones, the weak state is not a condition that has somehow simply happened. Continued weakness, whether in the separatist regions or in central governments, is in the interests of those in power.

There are also less egregious ways in which central governments respond to powerful disincentives to changing the status quo. Even politicians who may be committed, in good faith, to resolving the dispute must deal with radical domestic forces pushing in the opposite direction. In Georgia the Apkhazeti faction in parliament, the remnants of the former Georgian administration in Abkhazia that fled to Tbilisi during the war, has proved to be an obstacle to genuine compromise. The Apkhazeti, who enjoy set-aside seats in parliament, function as a regional government in exile; although they do not control enough parliamentary votes to challenge the strong government majority, they are vocal opponents of any move that looks to compromise their own interests in returning to power in Abkhazia.[59] Thus, they have long blocked legislation that would provide for resettlement and integration of the 250,000 people displaced during the Abkhaz war, people who have spent much of the past decade living in "temporary" accommodations in run-down hotels and resorts. Resettling the IDPs in Georgia proper, the faction leaders fear, would reduce their own political and economic power, since they control state budgetary disbursements to the IDPs and the provision of social services.[60] The Apkhazeti group has, in turn, proved a useful foil for the most independence-minded Abkhaz. Failed negotiations can always be blamed on the militaristic

[58] *Foreign Broadcast Information Service—Soviet Union* (October 24, 2000).
[59] *Georgia Today*, August 11–17, 2000, 3.
[60] United Nations interview (fn. 53).

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language of the Apkhazeti and on their supporters on the ground, the ethnic Georgian guerrilla movements that harass Abkhaz troops. The Abkhaz, the Apkhazeti, and the Georgian government—although radically distinct groups—have a common interest in obstructing real change.[61]

International Intervention as a Resource

In each of these conflicts, international involvement has been frequent if not frequently successful. In Azerbaijan the OSCE-sponsored Minsk Group has provided its good offices and a mechanism for negotiations since 1992. In Moldova an OSCE mission has been active since 1993 and has sponsored numerous rounds of negotiations. In Georgia a United Nations observer mission was deployed in 1993 to provide a basis for negotiations on Abkhazia's future and to monitor the peacekeeping operation conducted by the CIS forces in the Georgian-Abkhaz security zone. In South Ossetia, Russian peacekeepers have been in place since the end of the war, and negotiations on South Ossetia's final status have continued apace, involving Russia, North Ossetia, and the OSCE as mediators.

Despite this active engagement, little of significance has been achieved, for three broad reasons. First, in all cases the incumbent governments are arguing from positions of weakness. As the military losers in the conflicts, they have little to offer the separatist regimes. That basic dynamic is compounded by the parlous state of their own economies, which makes reintegration of little interest either to separatist elites or to their constituent populations. In all four disputes the separatists have insisted that full recognition of their independence should come first, after which they might be willing to negotiate some form of loose confederation with the incumbent governments. Central governments, by contrast, want precisely the opposite: an acceptance of state unity first, followed by discussions about devolution of power.

Second, because of the beneficial economies of stalemate, no key elites on either side have a major incentive to implement the agreements that have been signed. The belligerents have been favorably disposed to negotiate, even if scheduled sessions are routinely canceled or postponed, but rarely have the talks produced more than an agreement to keep talking—an outcome that seems acceptable to both sides. And

[61] Similar situations exist in Moldova (where pro-Romanian intellectuals have opposed concessions on Transnistria) and Armenia (where militants assassinated the prime minister in 1999, when he seemed to be moving toward a compromise with Azerbaijan).

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so long as the sides maintain "dialogue," they receive the political support and financial assistance of the international community. The major players have been willing to talk to each other precisely because the stakes are so low; few people on either side worry that what happens at the bargaining table will ever be implemented on the ground.[62]

Third, the policies of international negotiators have at times actually strengthened the statehood of the separatist regions. International intervention can itself be a useful resource for the builders of unrecognized states, for even accepting the separatist delegation as a negotiating partner confers some degree of legitimacy on that side's demands. And in more important and subtle ways, otherwise neutral facilitators have bolstered the separatists' hands. In Karabakh the difficulty of crossing the trenches between Karabakh and Azerbaijani forces—not to mention the excellent road link from Armenia—has meant that humanitarian and development programs, including those sponsored by the United States government, are managed from Armenia, not from Azerbaijan.[63] In Transnistria the local OSCE delegation strongly encouraged the Moldovan government to sign the agreements that provided customs stamps to the Transnistrians, thereby facilitating illegal commerce through the region. Later, the OSCE pressured the Moldovans to sign another accord that committed both sides to existence within a "common state," a form of language that the Transnistrians now interpret as Moldovan acquiescence to loose confederation.[64] In Abkhazia humanitarian relief agencies have become a pillar of the local economy, injecting as much as four to five million dollars into the economy each year through rents, services, and payment of local staff.[65] Even the most dedicated peacemakers thus find themselves in a no-win position: pushing an agreement with separatists who have no incentive to negotiate in good faith, with central leaders who benefit from the status quo, and with an impatient international community looking for any symbol of "progress," regardless of whether it actually contributes to resolution of the dispute.

[62] Author confidential interviews with senior OSCE and United Nations officials, Tbilisi, October 23 and 26, 2000.

[63] Author confidential interview with senior manager of United States assistance program, Stepanakert, September 28, 2000. Even the OSCE Minsk Group, the main negotiating forum, is based in Tbilisi, since basing the mission in either Baku or Yerevan would have been unacceptable to one of the sides.

[64] "Memorandum ob osnovakh normalizatsii otnoshenii mezhdu Respublikoi Moldova i Pridnestrov'em," signed Moscow, May 8, 1997.

[65] Author confidential interview with senior official in the United Nations Office for the Coordination of Humanitarian Assistance (UNOCHA), Tbilisi, August 29, 2000.

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IV. Peace as a Public Good

Eurasia's de facto countries are informational black holes. Traveling there is difficult and often dangerous. Elections have been held but never under the eyes of international observers. Economic and demographic data are not included in statistics compiled by national and international agencies. Books and newspapers barely circulate within the separatist regions themselves, much less to national capitals or abroad.

For all that, they may seem instances of what Freud called the narcissism of small differences. In most instances the leaders of these republics and their counterparts in central governments speak a common language—Russian—during negotiating sessions. Many had similar professional backgrounds during the Soviet period. The territory that separates them is in some cases minuscule: Tiraspol is fifty kilometers from the Moldovan capital, Chisinau; Tskhinvali is under two hours' drive from the Georgian capital, Tbilisi. Yet the problems they have spawned are immense. They are the central political problem for the recognized states whose territory they inhabit, and they have become conduits for trafficking in drugs, arms, and even people across Eurasia into Europe and beyond.

The strict security dimension of each of these conflicts—the threats posed by massive refugee flows or renewed fighting, say—is no longer a major concern. Since the end of the wars, separatist elites have got on with the process of building states, and even central elites and average citizens have learned to accommodate themselves to that process. But the cessation of the armed conflicts has perversely made a final political settlement even more difficult to achieve. Peace has now become something like a public good, an outcome from which all groups might potentially benefit but which entails some sacrifice from all interested parties. Just as the political economy of war can perpetuate violence, so too the institutions of Eurasia's unrecognized states have ensured that the benefits born of conflict continue to accrue to belligerents on both sides, the erstwhile losers as well as the winners.

To a certain degree, the energetic institution building in the separatist regions is a legacy of Soviet socialism. Three of the conflict zones had some of the basic institutions of statehood already (through their status as "autonomous" areas), and even in Transnistria local party organs and city councils provided the germ for what would later become a parliament, presidency, and security structure. The Soviet system provided a convenient template for how national issues ought to be channeled, a template that placed a premium on having and controlling

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statelike institutions drawn along national lines. It is indicative of the power of the Soviet legacy that among the first official acts of separatist elites was to set up a parliament and to adopt legislation on a national flag, anthem, and seal—long before they were even able to secure the territory they claimed as theirs. The supply of stateness in the Soviet system was there even before the demand.

Still, once the accoutrements of statehood have been put into place, they are extremely difficult to deconstruct. Why be mayor of a small city if you can be president of a country? Why be a lieutenant in someone else's army if you can be a general in your own? Of course, those calculations might be different if Azerbaijan, Moldova, and Georgia were strong, wealthy, or even marginally functional states, in which individuals in the separatist regions could see some advantage to reintegration. So far, however, life inside a recognized state (especially beyond the capital cities) is little different from life in one of the unrecognized ones. In some cases it is worse.

There is an obvious solution to this conundrum. Central governments could simply recognize the power of the separatist regions and opt for the maximum devolution of authority to them, in exchange for commitment to the existence of a single state. That has been the recommendation repeatedly put forward by the Russian Federation and generally supported by other external mediators, the idea of a final peace settlement based around the concept of a "common state" (*obshchee gosudarstvo*). As the Russian defense ministry's official history of these conflicts argues, the only possible course now is "the preservation of the existing de facto independent status of Abkhazia, Nagorno-Karabakh, and South Ossetia as juridically legitimate entities, as something like associated parts of internationally recognized states."[66] But even though this course might provide some diplomatic cover—a document that would allow the international community to claim that the conflicts had been "resolved"—it would do little to alter the basic structure of power. In fact, it would simply legitimize the continued division of these states into areas controlled by central governments and areas where their writ does not run. That may have been a workable solution in empires, where rebellious peripheral elites were granted tax-farming powers in exchange for loyalty to the center. It is not, however, a viable option for new, fragile, and allegedly democratizing states.

These dynamics call into question the academic lenses through which researchers have viewed the problems of intrastate war. Given

[66] Zolotarev (fn. 14), 395.

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the Western policy interest in the Balkans and the Caucasus in the 1990s, the study of conflict in these regions became of serious interest to security studies and, by extension, to international relations as a whole. Research has typically focused on the dimensions of conflict research derived largely from confrontations between states, such as the security dilemma. But seeing ethnoterritorial confrontations as mainly a security problem can blind researchers to the deep political and economic incentives that sustain disputes and fossilize networks of war into institutions of de facto states. The lesson of Eurasia's unrecognized countries is that these mechanisms are precisely where one should look to explain the conflicts' intractability. In civil wars, as in politics, asking cui bono can be illuminating.

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GLOBALIZATION, DOMESTIC POLITICS, AND SOCIAL SPENDING IN LATIN AMERICA

A Time-Series Cross-Section Analysis, 1973–97

By ROBERT R. KAUFMAN and ALEX SEGURA-UBIERGO*

"HAS globalization gone too far?" This question—also the title of a recent book by Dani Rodrik[1]—has been asked for over a century in Latin America. The issues it raises, however, have acquired special force in the last twenty-five years, as once-closed import-substituting economies have been transformed by structural reforms that have linked them far more closely to international trade and capital markets. As they do in other parts of the world, the specific effects of this transformation on Latin American societies remain unclear. Nevertheless, it seems quite apparent that it has brought about important modifications in the balance of political power and has altered the margins of choice available to domestic governments.

This article examines one of the most controversial aspects of this economic opening: its impact on governments' fiscal commitments to social security, health, and education. Many have argued that the new era of neoliberal reforms has undermined the thin protections that states in the region had provided to at least some of their citizens during earlier periods of ISI. Regardless of whether this is the case, the central challenge for the future is whether badly damaged welfare systems

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[1] See Rodrik, *Has Globalization Gone Too Far?* (Washington, D.C.: Institute for International Economics, 1997).

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can be reconstructed and expanded in ways that will shield citizens exposed to new market forces and enable them to compete effectively in the current era of globalization.

We explore these issues through an analysis of changes in social security transfers and of health and education expenditures in a time-series cross-sectional analysis in fourteen Latin American countries from 1973 to 1997. The countries are Argentina, Bolivia, Brazil, Chile, Costa Rica, Dominican Republic, Ecuador, El Salvador, Guatemala, Mexico, Paraguay, Peru, Uruguay, and Venezuela.[2]

We examine three sets of issues. First, has integration into global markets in fact constrained social spending? On this question, we draw heavily on the distinction drawn by Geoffrey Garrett[3] between an "efficiency" hypothesis, which posits that increasing exposure to international competition will induce governments to roll back social expenditures, and a "compensation" hypothesis, which emphasizes incentives to invest in human capital and to respond to political demands for protection against risk. We then examine the extent to which such outcomes might be influenced by two additional sets of domestic political and institutional factors: the balance of partisan power and the electoral pressures of democratic institutions.

Consistent with Garrett's findings for a larger global sample, we show that trade integration has a consistently negative effect on aggregate social spending and that this is compounded by openness to capital markets. This is the strongest and most robust finding in our study. Against at least some of the studies of OECD countries, moreover, the political variables have weak and inconsistent impacts on aggregate social spending. Neither popularly based governments nor democracies consistently spend more or less than conservative governments or autocratic regimes.

We also find, however, that globalization and domestic politics have a much more complex impact when social expenditures are disaggregated into social security transfers, on the one hand, and human capital spending on health and education, on the other. The negative effect of international economic integration operates primarily in the area of social security transfers (mainly pensions), while health and education expenditures are far less vulnerable. Each type of spending also appears to be influenced by different political factors. Popularly based governments

[2] Because of missing or noncomparable data, we were unable to include Colombia, Cuba, Haiti, Honduras, Nicaragua, and Panama.

[3] Garrett, "Globalization and Government Spending around the World" (Paper presented at the annual meeting of the American Political Science Association, Atlanta, September 1–5, 1999).

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tend to protect expenditures devoted to pensions and other welfare transfers, which have primarily benefited middle-class and union constituencies. The shift to democracy has a positive impact on health and education spending, which reaches a larger segment of the population.

Our study is distinctive, we believe, because it deploys broader measures of social spending than are found in most other samples of less developed countries and because these are examined on an annual basis over a relatively long period of time. In our analysis we use (1) a pooled time-series error-correction model, estimated through Ordinary Least Squares with panel-corrected standard errors to correct for panel heteroskedasticity and spatial correlation; (2) a lagged dependent variable to model the time dynamics and correct for serial correlation; and (3) country and time dummies to control for fixed effects. Compared with Generalized Least Squares (GLS) and Maximum Likelihood (ML) models, our procedure establishes a high threshold for estimating conventional levels of significance.[4] Such estimates are more reliable in the sense that the estimation of the standard errors is more efficient and consistent.

We present our analysis in the following steps. In the first section we outline the main theoretical arguments about how globalization and domestic politics might influence social spending in Latin America. The second section discusses the variables and the model used in the analysis. The third section presents our findings for changes in aggregate social spending. The fourth shows the impact of economic and political variables when spending is disaggregated into transfers, on the one hand, and into health and education expenditures, on the other. The last section presents the conclusions.

I. The Argument: Globalization, Domestic Politics, and Social Spending in Latin America

The "Efficiency" and "Compensation" Hypotheses

We begin by considering contending hypotheses about the effects of globalization on social spending. As Garrett[5] has noted, these reflect two quite contradictory sets of arguments that cannot be resolved without empirical research. Each offers very different propositions about

[4] Nathaniel Beck and Jonathan Katz, "What to Do (and Not to Do) with Time-Series Cross-Section Data," *American Political Science Review* 89 (September 1995); and idem, "Nuisance versus Substance: Specifying and Estimating Time-Series Cross-Section Models," *Political Analysis* 6 (July 1996).

[5] Garrett (fn. 3).

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the interests and resources of labor and capital and about the economic and political options that governments face.

The efficiency hypothesis rests on the assumption that high levels of social spending reduce competitiveness in global markets. This effect can operate through several channels. Increases in social spending might be linked, for example, to higher payroll taxes that increase the cost of labor and reduce the competitiveness both of exports and of domestic products exposed to import competition. Increases in fiscal expenditures can also undermine competitiveness by driving up interest rates, crowding out private investment, and increasing the value of the real exchange rate. Therefore, as business groups become increasingly exposed to international competition, they can be expected to press governments to reduce social expenditures. Integration into capital markets would presumably compound this pressure, since it increases the exit opportunities available to asset holders.

At the same time we might also expect a decline in labor's capacity to resist reductions in social spending. The Hecksher-Ohlin theorem, it is true, can be taken to imply the opposite: that in labor-abundant LDCs, the expansion of trade would lead to an increase in returns to labor and to an increase in its bargaining power vis-à-vis capital.[6] For a number of reasons, however, this has not generally been the case in Latin America. First, as Rodrik[7] argues with respect to LDCs in general, capitalists, having greater exit options than do workers, are in a better position to close their plants or relocate as the cost of labor increases. Against theoretical expectations, moreover, trade liberalization in many parts of Latin America has contributed to increasing demand for skilled workers, rather than for low-skill ones;[8] and even where this is not the case, the large pool of rural and informal sector workers creates a slack in the labor market that cannot be reduced quickly.[9] Finally, Latin American unions have been based in the public sector and import-substituting industries, both of which have been seriously weakened by trade liberalization. As economies become more exposed to international competition, therefore, the incentives for governments to curb social spending grow more powerful, while the political costs of doing so decline.

[6] Ronald Rogowski, *Commerce and Coalitions* (Princeton: Princeton University Press, 1989).

[7] Rodrik (fn. 1), 46.

[8] Barbara Stallings and Wilson Peres, *Growth, Employment and Equity: The Impact of Economic Reform in Latin America and the Caribbean* (Washington, D.C.: Brookings Institution Press, 2000).

[9] Nita Rudra, "Globalization and the Decline of the Welfare State in Less Developed Countries," *International Organization* (forthcoming).

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The compensation hypothesis posits just the reverse effect. It focuses on the welfare state as a mechanism for offsetting the social costs of international integration and for contributing to the development of human capital. In OECD countries this hypothesis is supported by studies that show a very strong empirical association between economic openness, large public sectors, and generous welfare systems.[10] Of course, we should not automatically expect similar developments in Latin America, where both factor endowments and political histories are obviously very different from those of the developed countries. Even so, studies by both Garrett and Rodrik show empirically that, as in developed countries, openness to trade leads to larger public economies in LDCs as well.[11]

There are several reasons why the unsettling effects of increasing international competition might lead governments of LDCs to expand commitments to social spending. First, regardless of their specific role in the international economy or the net economic gains brought about by trade liberalization, countries that increase their exposure to international markets are likely to experience social dislocations, uncertainty, and unequal distributive effects. This in turn creates a potential for political instability and/or backlash against market-oriented economic policies. Governments and businesses would have an incentive to keep these threats at bay by providing welfare transfers to social sectors or geographic regions that had fallen behind in the process of change.

As in developed countries, moreover, increasing exposure to trade may also strengthen incentives to use social spending to enhance the skill level and productivity of the labor force. To the extent that public investment in human capital provides a collective good for the private sector, business groups might welcome or even press for these expenditures. This is because when large welfare states enhance labor skills and ensure political stability, they may provide collective goods that enhance the competitiveness of the economy in international markets. We therefore note here that the term "compensation hypothesis" is rather misleading if one assumes that expanding social spending is necessarily less efficient than cutting it back.

[10] See, for example, David Cameron, "The Expansion of the Public Economy: A Comparative Analysis," *American Political Science Review* 72 (December 1978); and Peter Katzenstein, *Small States in World Markets* (Ithaca, N.Y.: Cornell University Press, 1985).

[11] Garrett (fn. 3); and Rodrik (fn. 1).

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Domestic Politics

Whether governments adopt efficiency or compensation strategies may also depend on the means citizens have to mobilize around economic interests and to hold governments accountable. Two additional sets of political and institutional factors may therefore also influence social spending as economies become more open. One of these is the balance of power among interest groups and party organizations. In the OECD countries strong unions and social democratic governments have often been the paramount forces behind the expansion of welfare systems. Conceivably, they are also important forces for resisting cutbacks, although this is a matter of some dispute in the OECD cases.[12]

In Latin America, as in other LDCs, unions are notoriously weak, at least in comparison with their counterparts in Western Europe; moreover, cross-national differences within the region are extremely difficult to measure systematically. A recent study by Nita Rudra[13] attempts to circumvent the measurement problem by focusing on variations in labor-market conditions as a proxy for the bargaining power of organized labor. In a global sample of LDCs, she finds that social security spending varies positively with the ratio of skilled to unskilled labor and varies negatively with the pool of surplus labor. Nonetheless, still lacking are the more direct and reliable indicators of organizational strength (membership, cohesion, and so on) that characterize studies of the OECD.

Our more direct approach to this problem focuses on the political orientation and constituent base of the parties supporting incumbent presidents. As we discuss below, the social security transfers advocated by popularly oriented parties in Latin America sometimes benefit their labor constituencies, but they may also have a negative impact on the incomes of rural and informal sector workers. Even when the transfers pursued by such parties do not reach the very poor, however, we can hypothesize that social spending is more likely to be sustained under presidents who have been elected with their support.

Finally, within the Latin American and LDC context, we need to ask more explicitly whether democracy itself makes a difference—a matter of some dispute in the literature on economic and social reform. One view is that the general distinction does not have much explanatory sig-

[12] Paul Pierson, "The New Politics of the Welfare State," *World Politics* 48 (January 1996); and Alexander Hicks, *Social Democracy and Welfare Capitalism: A Century of Income Security Politics* (Ithaca, N.Y.: Cornell University Press, 1999).

[13] Rudra (fn. 9).

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nificance and that it is more important to focus on more specific features of constitutional design, party systems, and partisan politics.[14] An alternative perspective rests on a relatively straightforward theoretical point: democratic rulers face pressures from a mass electorate to deliver social services and are thus more likely than authoritarian rulers to respond to demands for compensation.

Resolving this issue is complicated by the fact that, as noted above, many social services are inequitably distributed in Latin America.[15] Nevertheless, they do reach significant portions of the middle- and working-class population—those social sectors most likely to turn out at the polls. For this reason, the distinction between electoral democracies and autocracies may be a potentially important causal factor in spending decisions. In fact, a recent study by Brown and Hunter[16] shows that Latin American democracies are more likely to maintain social security, health, and education expenditures in the face of economic downturns. We in turn build on their work by asking whether the impact of political regimes is affected by integration into international markets.

The Latin American Sample

These issues and related questions have received considerable attention in quantitative studies of OECD countries, and more recently they have been explored in global samples.[17] Our Latin American sample cannot draw on the refined data sets available for the OECD and it lacks the wide empirical scope of the broader samples. However, there are advantages to focusing on the countries of Latin America.

First, unlike many other LDCs and transition economies, many Latin American countries have long had occupationally based welfare systems modeled along European lines, with defined-benefit pension plans, health services, and family allowances. By the 1920s the groundwork

[14] Stephan Haggard and Robert R. Kaufman, *The Political Economy of Democratic Transitions* (Princeton: Princeton University Press, 1995).

[15] The distributional impact of social spending is still subject to empirical debate. One important study conducted under the auspices of the UN Economic Commission on Latin America and the Caribbean finds that social spending had a positive impact on the lowest quintile in Argentina, Brazil, Chile, and Colombia. See Rossana Mostajo, "Gasto social y distribución del ingreso: caracterización e impacto redistributivo en países seleccionados de América Latina y el Caribe," *Serie Reformas Económicas* 69 (Santiago: ECLAC, 2000).

[16] David S. Brown and Wendy Hunter, "Democracy and Social Spending in Latin America, 1980–92," *American Political Science Review* 93 (December 1999). See also George Filho Avelino, "Economic Crisis, Democratization, and Social Expenditure in Latin America, 1980–1994" (Ph.D. diss., Stanford University, 2000).

[17] See the studies by Garrett (fn. 3); Rodrik (fn. 1); and Rudra (fn. 9).

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for these systems had been established in Argentina, Uruguay, and Chile. During the 1930s and 1940s a second wave of countries followed suit, including Brazil, Costa Rica, Mexico, Venezuela, Panama, and Colombia.[18]

Notwithstanding distributive inefficiencies and inequities, social safety nets and services covered significant portions of their respective societies. By the 1980s estimates were that coverage reached from 62 to 96 percent of the economically active population in at least five countries (Uruguay, Argentina, Chile, Brazil, and Costa Rica) and from 45 to 53 percent of the same population in Panama, Mexico, and Venezuela.[19] By the standards of developed countries, of course, this was not a very good record. Nevertheless, at the onset of Latin America's "great transformation," such welfare systems constituted an important part of the "social contract" connecting citizens with the state.

The Latin American sample is also interesting because of the political transformations that have swept the region over the past two decades. Latin American countries were among the first non-European states to join the third wave of democratization. The fact that these political transitions occurred more or less concurrently with economic openings gives special salience to the question of whether democracies can mitigate the potential negative effects of globalization.

Finally, the limited cross-national scope of our sample is partly offset by the coverage and reliability of the available data. This is particularly important with respect to the measurement of the dependent variable. Although coverage of social spending contains a number of problems, it does add some important dimensions to other studies of LDCs. The aggregate measures of government spending used in Rodrik's and Garrett's[20] global samples are imperfect substitutes for social spending, at least in Latin America. Measured as a percentage of the GDP, central government spending and social spending are highly correlated (.81); but the simple correlation between government spending and social spending per capita is only .51, and it is even lower (.31) with the share of social spending in the public budget. Finally, spending data in several other important studies have not included health and education expenditures, or they cover a more limited period of time.

As with these other measures, the validity of our data is compro-

[18] Carmelo Mesa-Lago, *Social Security in Latin America: Pressure Groups, Stratification, and Inequality* (Pittsburgh, Pa.: University of Pittsburgh Press, 1978).

[19] Carmelo Mesa-Lago *Ascent to Bankruptcy: Financing Social Security in Latin America* (Pittsburgh, Pa.: Pittsburgh University Press, 1989), 41.

[20] Garrett (fn. 3); and Rodrik (fn. 1).

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mised by the fact that some of the most serious problems of LDC welfare systems involve defects in the organization and distribution of benefits, rather than in financing per se.[21] For this reason, as we have suggested, all types of spending measures are imperfect proxies for the actual payoffs that citizens receive. It is plausible to assume, however, that even relatively efficient and equitable delivery systems will require significant financial commitments from the public sector. The spending measures used in this study and others provide at least a rough indication of the resources governments are prepared to devote to social needs.

II. The Variables and the Model

The Variables

SOCIAL SPENDING

Our social spending variables are based on annual IMF data for public spending on social security, health care, and education.[22] These expenditures in turn are measured in three ways: in per capita 1995 dollars, as a percentage of GDP, and as a share of central government spending net of interest payments on the public debt. Interest payments are excluded from government spending totals, because they are in part the product of accumulated long-term debt that cannot be reduced quickly by the government in power, whereas we are interested in how these governments establish budget priorities. We present findings for each specification of the dependent variable, because each captures different kinds of welfare effort. As just noted, the fiscal share of social expenditures reflects priorities set within the public sector. Spending as a percentage of GDP indicates allocative priorities within the national economy as a whole. Welfare dollars per capita measure the value of the resources potentially available to recipients.

Unfortunately, as in other large-N studies of public spending in developing countries, annual data are available only for central government spending.[23] This presents a serious problem for our analysis,

[21] Evelyne Huber, "Options for Social Policy in Latin America: Neoliberal versus Social Democratic Models," in Gosta Esping-Andersen, ed., *Welfare States in Transition: National Adaptations in Global Economies* (London: Sage Publications, 1996); and Joan Nelson, "The Politics of Pension and Health-Care Delivery: Reforms in Hungary and Poland," in Janos Kornai, Stephan Haggard, and Robert R. Kaufman, eds., *Reforming the State: Fiscal and Welfare Reform in Post-Socialist Countries* (New York: Cambridge University Press, 2000).

[22] International Monetary Fund, *Government Finance Statistics* (Washington, D.C.: IMF, various years).

[23] Garrett (fn. 3) uses general government consumption expenditures, but this is based on cross-sectional averages and does not include transfer payments.

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because in the late 1980s a number of federal systems began shifting some responsibility for social programs to state governments. We deal with this problem in a number of ways, although none is fully satisfactory. First, while the main impulses for fiscal decentralization did not occur in most countries until the 1990s, our data set extends back to the early 1970s. And even with decentralization, federal governments retained responsibility for pensions and many other social services. Finally, we find no important differences in the results of our model when we exclude Brazil and Argentina, the two most decentralized countries during most of the period covered in the model.

After declining during the fiscal crises of the 1980s, social spending within Latin America as a whole rose substantially during the 1990s, a period in which the region also became increasingly integrated into the world economy. On the surface the concurrence of spending increases and economic opening would appear to support the compensation hypothesis. It is impossible to assess the causal connection between the two trends, however, without also taking account of the effects of other factors that can also influence social spending. That is what we do in this article.

It is important to emphasize, moreover, that the rate of change varied considerably from one country to the next. Over the twenty-five-year period covered by our model, the average annual change in spending per capita was $7.30, whereas the standard deviation was $52.39. The changes as a percentage of the budget and of GDP averaged .16 and .08 percent, respectively, while the standard deviations were 4.82 and 1.31. During the upward trend of the 1990s annual rates of change varied from a low of −1.7 percent in Honduras to 22 percent in Peru, and even by the end of the decade, spending in El Salvador, Guatemala, and Venezuela remained below pre-1980 highs.[24] So there is enormous variation in the dependent variable.

GLOBALIZATION

Exposure to international markets is measured in two ways. Following conventional practices in most of the literature on globalization,[25] trade

[24] Economic and Social Commission on Latin America and the Caribbean (ECLAC), *Social Panorama of Latin America* (Santiago, Chile: ECLAC, 1999).

[25] See, for example, Alexander M. Hicks and Duane H. Swank, "Politics, Institutions and Welfare Spending in Industrialized Democracies, 1960–1982," *American Political Science Review* 86 (September 1992); Evelyne Huber and John Stephens, *Development and Crisis of the Welfare State: Parties and Policies in Global Markets* (Chicago: University of Chicago Press, 2001); Torben Iversen and Thomas R. Cusack, "The Causes of Welfare State Expansion: Deindustrialization or Globalization?" *World Politics* 52 (April 2000); Rodrik (fn. 1); and Garrett (fn. 3).

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integration is calculated as imports + exports/GDP. This measure is affected by the size of the economy and by changes in the exchange rate, but the inclusion of country dummies and exchange-rate variables as regressors in our model corrects for these effects.

For openness to international capital markets, we use an index of capital account liberalization developed by Morley, Machado, and Pettinato[26] that reflects (1) the extent of sectoral control on foreign investment, (2) limits on profit and interest repatriation, and (3) controls on external credits by national borrowers and capital outflows. We use this policy index instead of a more direct measure of capital flows, because flows often indicate macroeconomic volatility rather than openness, especially in such an unstable region as Latin America. As with the use of trade ratios, this choice follows a practice common in the literature on globalization.[27]

POPULARLY BASED PRESIDENTS

To gauge the relative balance of partisan power, finally, we have coded all democratic heads of state in terms of the political orientation of their party base. Presidents are coded as popularly based if their parties have close historical links with labor unions (for example, the Peronists in Argentina or Acción Democrática in Venezuela) and/or if their parties have long-standing programmatic orientations toward "the popular sector" (for example, the MNR in Bolivia or the PLN in Costa Rica). It is important to emphasize that our coding deliberately does *not* take into account whether individual presidents themselves were conservative or left leaning in their own social policy preferences; in fact, some popularly based leaders like Carlos Saúl Menem lean decisively to the right. The question, however, is whether their policy behavior is constrained by their constituent base or partisan supporters—an issue that should be resolved empirically, rather than by definition. Again, this approach parallels a question typically asked about OECD countries, namely, whether parties of the left behave differently from conservative ones once they arrive in government.

We have also coded a number of autocratic regimes as popularly based, according to the way specialists have characterized their principal support coalitions or their strategies for building political support. One example is the military regime that took power in Peru in 1968; a

[26] Samuel Morley, Roberto Machado, and Stefano Pettinato, "Indexes of Structural Reform in Latin America" (Santiago, Chile: ECLAC Economic Development Division, LC/L.1166, January 1999).

[27] See Garrett's discussion of these issues (fn. 3); and Dennis Quinn, "The Correlates of Change in International Financial Regulation," *American Political Science Review* 91 (September 1997).

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second is the dominant-party regime in Mexico. Although we have more confidence in the validity of this coding in democratic regimes, it is of interest to see whether popular bases determine behavior independently of regime type.

DEMOCRACY

Finally, following Alvarez, Cheibub, Limongi, and Przeworski, we use a dichotomous measure of democracy, based on Keith and Gurr's Polity III data set.[28] Countries were ranked by subtracting the 10-point "autocracy" scale from the 10-point "democracy" scale. Any country scoring at least 6 points is coded as democratic, and the rest are coded as authoritarian.[29] As noted in the preceding section, we would expect democratically elected governments to have a positive effect on changes in welfare spending as their countries become more integrated into the international economy.

CONTROL VARIABLES

We have examined the impact of a large number of control variables, including population size, urbanization, public debt, government revenues, dummies for inflation, exchange rate, and GDP shocks, logged GDP, and GDP growth. Most of these were excluded from the final model in order to avoid problems of multicollinearity and to enhance the clarity of our presentation. Our eventual choices of which controls to include in the final model were based on the strength of our initial theoretical expectations, the completeness of data coverage, and the Chow and Aiken information tests to determine the contribution of the controls to the total variance explained in the model. It should be emphasized, however, that none of the controls excluded from this model altered our basic substantive findings.

The final specification of the model incorporates the effects of demographic composition by including controls for the age of the population or, where relevant, the percentage of dependents who are children or elderly. GDP/capita controls for Wagner's law, which holds that the size of government increases with the wealth of the economy. We also

[28] Michael Álvarez, Jose Antonio Cheibub, Fernando Limongi, and Adam Przeworski, "Classifying Political Regimes," *Studies in Comparative International Development* 31 (Summer 1996); and Keith Jaggers and Ted Robert Gurr, *Polity III: Regime Type and Political Authority, 1800–1994* (ssdc.ucsd.edu/ssdc/icp06695.html), consulted September 2000.

[29] We have also tried other specifications of democracy such as (1) using a continuous measure or (2) changing the cutting point from 6 to 7 or 5. We did not see any significant changes in the results.

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include an output gap variable used by OECD economists. Derived by comparing the actual value of GDP in a given year with the value predicted by the underlying growth trend, it can be used to assess the effects of the business cycle on social spending. A positive sign would indicate that these effects are procycle, whereas a negative relation would show a countercyclical pattern. As a control, the output gap measure eliminates the possibility that the effects of other variables are actually caused by these cyclical relationships.

Since changes in social spending may actually be an effect of more general changes in government expenditure, we include the latter as another control; and exchange-rate fluctuations are also included to take account of their possible effects on trade and capital account openness. Following Krugman,[30] we estimate the real exchange rate by multiplying the nominal rate in each country by the ratio of local consumer price inflation to the U.S. CPI index. A more complete description of all of these variables is provided in Appendix 1.

Finally, the model also takes into account the impact of both time and fixed effects. Decade dummies are used to account for the important differences in regional and international conditions over the course of our time period. The first covers 1973 to 1981, the years prior to the debt crisis. The second extends from 1982 to 1990, years that were generally marked by economic recession and painful structural adjustments. The last covers the period of economic recovery that took place during the first half of the 1990s.[31] Country dummies are included in all of the specifications of the model. These correct for factors that might impact a country's economic openness and/or welfare spending over the long run, such as the size of the population and territory, wealth, and long-term political history.

The Model

Our construction of the time-series model takes into account the important distinction between the analysis of cross-national differences and the analysis of changes within individual countries over time.[32] Cross-national differences in the size of the welfare state are likely to be invariant over time, because they are influenced by historical factors at work over long periods or by structural conditions that change only

[30] Paul Krugman, *International Economics* (New York: Addison-Wesley Longman, 1999).

[31] We also ran the regressions substituting year dummies for decade dummies. This did not significantly affect the results.

[32] See Garrett (fn. 3); and Huber and Stephens (fn. 25).

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slowly. The causes of such differences are best assessed statistically through analyses in which the key explanatory variables (openness, left strength, and so on) are expressed as long-term properties of the system.[33] In this connection, we mention in passing that, in contrast to the OECD cases, cross-sectional OLS regressions for Latin America show no significant relationship between openness and the size of government. Some countries with open economies, such as Panama, do have large governments, but many other small, open Central American societies do not. These results, moreover, are unaffected by controls for GDP and democracy.

In this article, however, we are interested in *changes* in social spending, which are presumably influenced more directly by dynamic processes of globalization and by contemporaneous political pressures. We use an error-correction model that is well suited for just such a purpose. As discussed in the introduction, we have taken particular care to deal with the most common problems that affect time-series cross-sectional models. We have followed the methodology suggested by Beck and Katz,[34] whereby the use of Ordinary Least Squares with panel-corrected standard errors deals with the problem of panel heteroskedasticity and spatial correlation and the lagged dependent variable corrects for serial correlation.

We use country dummy variables and time dummies to control for country-specific and time-specific fixed effects. The use of fixed effects is becoming the norm in panel studies of the welfare state and is particularly important in our model, since most variables vary more across units than over time. The use of panel-corrected standard errors usually produces rather conservative results, since it tends to increase the standard errors of the estimates. Moreover, the inclusion of dummy variables tends to deflate the statistical significance of the other regressors.[35] While this method carries some risk that causal hypotheses will be rejected prematurely, it also increases our confidence that results which do emerge as significant are not the consequence of unsound statistical assumptions or inappropriate econometric methods.[36]

[33] Garrett (fn. 3).

[34] Beck and Katz (fn. 4).

[35] Lois Sayrs, *Pooled Time Series Analysis* (London: Sage Publications, 1989).

[36] The failure to address these technical problems has called into question the findings of a number of earlier studies. For example, in a replication of Hicks and Swank's (fn. 25) influential study of OECD spending, only four of thirteen political and institutional variables reach conventional levels of significance when panel corrected standard errors are used; see Beck and Katz (fn. 4).

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The generic version of the model can be specified as[37]

$$\Delta Y_{i,t} = D\alpha + Y_{i,t-1}\cdot\beta_0 + \Delta X_{i,t-1}\cdot\beta_k + X_{i,t-1}\cdot\beta_j + T\lambda + \varepsilon_{i,t} \qquad (1)$$

where $Y_{i,t}$ is welfare expenditures in country *i* during year *t*, X is a vector of independent variables, D is a vector of country dummy variables or fixed effects, and T is a vector of time effects. Specifications of the dependent variable are measured as first-differences, and the independent variables include the lagged level of welfare expenditures, the lagged level of each independent variable, and the yearly changes (Δ) in the independent variables.

This type of model is based on the idea that the dependent and independent variables are in a long-run equilibrium relationship but that there are also important short-term or temporary effects.[38] As noted above, the Δ variables on the right-hand side of the equation measure first-difference changes that are used to estimate annual changes in the dependent variable. Their overall impact on spending depends on the magnitude of the regression coefficient (β_k) associated with the first-difference variable and the extent to which the change persists over time, which in turn depends on the coefficient of the lagged dependent variable (ϕ). In other words, if a 10 percent change in Δ trade is sustained in subsequent years, the effect will be larger than if the change is subsequently reversed.

The coefficients (β_j) of the *levels* variables ($X_{i,t-1}$) measure long-term effects on the dependent variable. They allow us to assess whether trends in the independent variable are causally related to long-term trends in the dependent variable. When the regression coefficient (β_j) is statistically significant, it indicates that there is a long-term causal relationship between these trends. The strength of that relationship is estimated by dividing the regression coefficient (β_j) by ($-\phi$), the yearly rate at which the unpredicted annual changes in the Y variable return to the trend line (see Appendix 2).

The inclusion of both first-difference and levels variables is a statistical requirement of the error-correction model. The interpretation of their causal role, however, requires theoretical and conceptual judg-

[37] This model is equivalent to the one described by Beck and Katz (fn. 4), in which the authors explain the importance of separating short-term from long-term effects in dynamic models (see Appendix 2).

[38] See William Greene, *Econometric Analysis,* 4th ed. (Upper Saddle River, N. J.: Prentice Hall, 2000), 733–35; Anindya Banerjee, Juan Dolado, John Galbraith, and David Henry, *Co-Integration, Error Correction, and the Econometric Analysis of Non-Stationary Data* (Oxford: Oxford University Press, 1993).

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ments. The demographic measures used as control variables, for example, change slowly from year to year, and their effects are most likely to work through the levels variables. In other cases both first-difference changes and long-term trends may have substantive meaning. Thus, for example, the first-difference variables for democracy or popularly based governments can be presumed to measure the effects of a regime transition or change of government in a given year, while the levels variables measure the longer-term effect of these changes within a given country. To the extent that the effects of trade on spending work through the lobbying efforts of business groups exposed to international competition, they are most likely to be felt over the long term. As we suggest below, however, governments that link structural reforms to spending reductions may also produce important short-term effects captured by first-difference variables.

III. Results for Aggregate Social Spending

The estimates for changes in aggregate levels of spending are shown in Table 1. To enhance the clarity of presentation, we do not include country dummies in the table and also do not show the first-difference effects of several control variables that would appear to work primarily as longer-term trends. Overall, the models explain between about 35 and 46 percent of the variance in social spending. This is a reasonably good fit. A model using only levels variables typically leads to much higher R^2, but this is only because the lagged dependent variable artificially inflates the total variance explained.

Coefficients of the control variables go in the expected direction most of the time and are uniformly consistent with expectations when they reach standard thresholds of statistical significance. Not surprisingly, trends in central government spending have an important impact: as the size of the central government increases, so does welfare spending per capita and as a percentage of GDP. The impact of the output gap is significant for spending/GDP and positive in the other specifications, an indication that social spending in Latin America tends to be procyclical. The coefficients for the decade dummies measure the effects on spending in the 1970s and 1980s, compared with that of the 1990s, the omitted category. As expected, the fact that these coefficients are negative and significant indicates that, when all else is held constant, spending in the 1970s and 1980s was lower than in the 1990s. As noted, moreover, even where the individual control variables do not reach standard significance levels, Chow tests and Aiken information criteria

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TABLE 1
DETERMINANTS OF SOCIAL SPENDING IN
14 LATIN AMERICAN COUNTRIES[a]
(1973–97)

	D.welfcap	*D.welfpub*	*D.welfgdp*
L. GDPCAP	0.04578***	0.00084	−0.00015
	(2.95)	(0.77)	(0.65)
L. OUTPUT GAP	1.16775	0.18411	0.08660***
	(0.86)	(1.02)	(2.64)
L. AGE65	26.62502**	−0.72316	0.16036
	(2.56)	(0.66)	(0.82)
L. GOVERNMENT	2.91540***	0.24923	0.08819***
	(3.22)	(1.38)	(3.72)
D. GOVERNMENT	10.19899***	−0.20322	0.26326***
	(8.92)	(0.69)	(10.68)
L. EX.RATE	0.00665	−0.00168	−0.00022
	(0.92)	(1.08)	(1.13)
D. EX.RATE	0.02540	0.00278	0.00070*
	(1.56)	(0.75)	(1.66)
L. TRADE	−0.89904***	−0.07027**	−0.01662**
	(3.04)	(1.99)	(2.48)
D. TRADE	−1.47504***	−0.10286**	−0.03935***
	(4.61)	(2.30)	(5.05)
L. CAPITAL	−0.44671*	0.06978**	−0.00007
	(1.89)	(2.21)	(0.01)
D. CAPITAL	0.20770	0.09792*	0.00104
	(0.51)	(1.78)	(0.13)
L. DEMOCRACY	−12.32505	0.08369	−0.19032
	(1.52)	(0.06)	(1.04)
D. DEMOCRACY	−15.96276	1.05218	−0.52492
	(1.05)	(0.40)	(1.59)
L. POPULAR	−11.20824*	1.17233	−0.04741
	(1.73)	(1.18)	(0.32)
D. POPULAR	2.07271	2.41128**	0.04107
	(0.29)	(2.11)	(0.25)
Dec-70	−38.24258***	−6.36836***	−0.92348***
	(3.41)	(3.46)	(3.40)
Dec-80	−36.30183***	−4.56418***	−0.93233***
	(4.61)	(3.14)	(4.85)
Lagged DV	−0.28097***	−0.61602***	−0.27955***
	(3.14)	(4.78)	(4.28)
Constant	−378.22368***	32.19257**	1.81409
	(2.63)	(2.42)	(0.66)

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TABLE 1 *(cont.)*

	D.welfcap	*D.welfpub*	*D.welfgdp*
R-squared	0.4600	0.3589	0.4567
Prob>Wald Chi2	0.0000	0.0001	0.0000
Observations	284	284	284

* significant at 10%; ** significant at 5%; *** significant at 1%; panel-corrected z-statistics in parentheses

[a]In Tables 1–4 variables preceded by "L" are measured in levels; variables preceded by "D" are measured in first differences (the first differences of the first three control variables were, in most cases, not significant and are therefore not shown). Estimation is made with an error correction model that is robust to unit roots (see Appendix 2). The Lagrange multiplier test indicates that the model is not affected by serial correlation. The model is estimated with fixed effects (Least Squares Dummy Variables model). To save space, country dummies are not shown. The country dummy for Argentina was omitted to avoid perfect collinearity. An F-test for the significance of the fixed effects indicated that, at a 5 percent (or better) level of significance, the fixed effects belong in the model. The correlation between the fixed effects and the regressors is about 0.8. Hence, the model cannot be estimated with random effects. The model was estimated with the "xtpcse" command in STATA 7.0 Alternative estimation techniques such as "rreg" (robust regression), maximum likelihood, generalized method of moments, or the Arellano-Bond estimator did not produce substantive changes in our main results. We therefore preferred to use OLS with panel-corrected errors, as suggested by Beck and Katz (fn. 4, 1995 and 1996), due to its relative simplicity and the ease of interpreting results.

show that they generally contribute significantly to the overall variance in the model.

Turning now to the substantive variables highlighted in the general discussion of globalization, the most striking finding is the strong negative effects of trade openings. Both the long- and short-term effects cross the high thresholds of significance of the error-correction model across all specifications of the dependent variable. The model shows that the effects of trade integration are independent of the conjunctural circumstances of the 1980s and 1990s, the business cycle, demographic changes, exchange-rate fluctuations, and fixed country effects. The trade variables are also robust against a battery of other controls that we eventually chose not to include in the model, including inflation, inflation crises, exchange-rate shocks, and sudden drops in the GDP.[39]

The coefficients for trade levels can be interpreted as an indication

[39] A sequential series of regressions, which excluded one country at a time, shows that these results are not driven by any given country. To check for possible outliers, we used robust regressions that use D-beta and Cook distances to correct for unusually deviant observations; the results obtained were very similar.

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that secular shifts in the preferences and relative power of business sectors exposed to increases in international competition curb social spending over the long term. In that respect, they are quite consistent with the efficiency hypothesis. As discussed in Appendix 2, the substantive impact of this variable is estimated by dividing the regression coefficient (β_j) by the negative value of the lagged dependent variable (ϕ). With all else held constant, a 10 percent increase in long-term trade between 1973 and 1997 produces an average decrease of over $31 per capita in social spending; that comes to over 10 percent of the regional average of $255. At least five countries (Chile, Costa Rica, the Dominican Republic, Mexico, and Uruguay) experienced much larger increases—from 25 to 50 percent—in the ratio of trade to GDP. In those cases the predicted decrease in spending would range from about $78 to $156. Under the same conditions, spending would decline by about 1.5 to 3.0 percent of GDP; and between 2.9 and 5.8 percent as a share of government spending, against regional averages of about 7.0 and 46 percent, respectively.

The short-term effect of an increase in trade is also important, and the cumulative impact can be quite substantial if it is sustained over time. For example, there were twenty-two instances in which countries experienced at least a 10-point increase in their ratio of trade to GDP. We can simulate the effects of such an increase in Mexico, which is more or less at the mean of the sample in terms of social spending and where trade actually grew by an even greater amount in 1995. Holding all else constant but assuming a one-year 10-point increase in trade, spending during the first year would decline by about $15 per capita, by 0.4 percent of the GDP, and by a more modest 1 percent of government spending. If the onetime increase in trade is subsequently sustained (as it was in Mexico), the cumulative effect after five years leads to a decline in spending from $200 to about $155 per capita, and from 6.3 to 5.1 percent of GDP. The cumulative drop in budget share is considerably smaller, but still slips from 46.6 to 45 percent.[40]

Although these first-difference effects are generally strong, they are difficult to interpret in the same way as the long-term effects of the levels variables. Whereas the latter may reflect direct or anticipated pressure of business groups, it seems less likely that such pressures would be mobilized or anticipated so quickly on a year-to-year basis. An alternative explanation is that the short-term effects reflect assumptions made by policymakers themselves about the relation between structural re-

[40] See Appendix 1 for the formula used in these calculations.

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form and fiscal adjustment. If liberal reformers view such adjustments as a necessary condition of efficient trade competition, for example, they might initiate curbs on social spending independently of pressure from business interests, conceivably as a component of an overall reform package.

As a check on this possibility, we reestimated our model, replacing our trade-ratio variable with an index of trade policy liberalization developed by Morley, Machado, and Pettinato.[41] The effects of the first-difference changes in trade policy were remarkably similar to Δ trade, whereas the coefficients for the long-term effects of policy were not significant. The implication is that different causal mechanisms drive social spending reductions in the short and the long run. In the short run these reductions result from the initiatives of macroeconomic policy elites. If these reforms are sustained and trade expands over the longer run, the downward pressures on spending may reflect structural changes in the economy and the broader interests of producer groups themselves.

Unlike trade, capital account liberalization does not have a consistent impact on social spending (see Table 1); the significance of the effects is sensitive to the other variables included in the model and the signs are not robust across alternative specifications of social spending.[42] One implication is that the main pressure for reducing expenditures comes from producer interests exposed to competition, rather than from a more general concern with establishing credibility in international financial markets. For producers, increases in welfare expenses imply higher payroll taxes, which have a direct impact on their bottom line. They therefore have an incentive to lobby directly against expanding welfare commitments.[43] Holders of liquid assets worry more about aggregate macroeconomic fundamentals than about welfare expenditures or labor costs per se.

The reduction of controls on the movement of capital, however, does expand exit options for producers; and when capital openness is interacted with trade increases (see Table 2), the long-term effects of the interaction *are* consistently negative and reach standard levels of significance for spending per capita and as a share of the budget.[44]

Figure 1 illustrates how to interpret the impact of the interaction co-

[41] Morley, Machado, and Pettinato (fn. 26).

[42] Quinn (fn. 27) also finds a positive relation in his study of OECD countries, as does Garrett (fn. 3) in his global sample.

[43] See Jeffry Frieden, *Debt, Development, and Democracy* (Princeton: Princeton University Press, 1991); and Rodrik (fn. 1).

[44] Note that in the interaction model, the simple coefficients for trade and capital are necessary as

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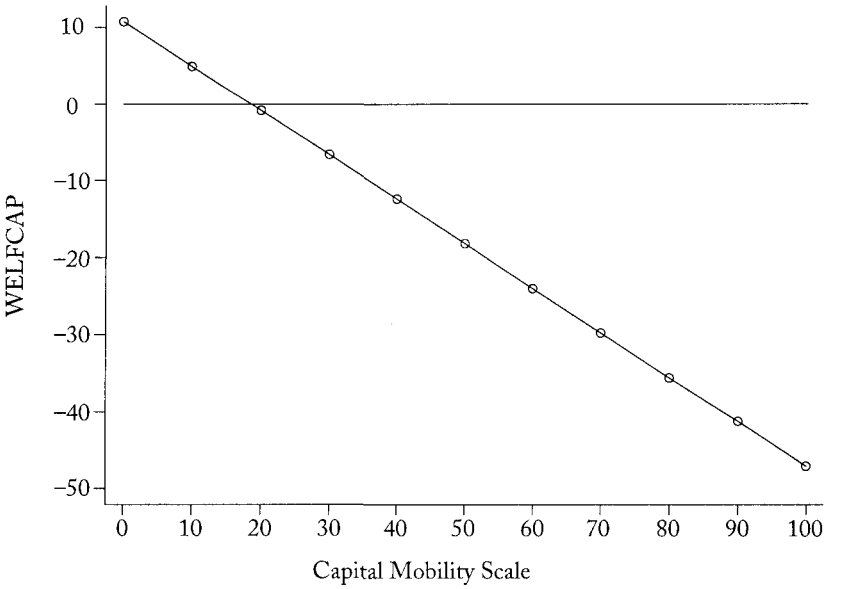

FIGURE 1
CONDITIONAL EFFECT OF A 10% INCREASE IN TRADE ON SOCIAL SPENDING PER CAPITA, CONDITIONAL UPON DEGREE OF CAPITAL MOBILITY[a]

[a]This figure has been created using the interaction coefficient between the level variable for trade and the level variable for capital mobility (see Table 2). The uninteracted coefficient for trade (0.3124) measures the impact of trade on social spending when capital mobility is 0. To capture long-term effects, the conditional coefficient needs to be divided by the lagged dependent variable (in absolute terms) and then multiplied by the pertinent level of capital mobility.

efficients shown in Table 2.[45] The figure unpacks these coefficients and allows us to trace the way trade increases affect spending at different levels of capital-account openness. The x-axis plots the index of capital openness from 0 to 100. At every 10-point interval on this scale, we estimate the effect of a 10 percent increase in trade on per capita social spending; these estimates are plotted on the y-axis.

The figure shows that if the capital account were totally closed, a 10 percent expansion of trade might have a positive effect on spending; but

controls but substantively meaningless. The coefficient for each uninteracted variable measures its impact when the value of the other variable is zero. See Robert Friedrich, "In Defense of Multiplicative Terms," *American Journal of Political Science* 26 (November 1982).

[45] We are grateful to William Roberts Clark, Department of Politics, New York University, for his methodological advice and assistance in this portion of the paper.

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TABLE 2
DETERMINANTS OF SOCIAL SPENDING IN 14 LATIN AMERICAN COUNTRIES, 1973–97[a]
(INCLUDES AN INTERACTION COEFFICIENT BETWEEN TRADE AND CAPITAL MOBILITY)

	D.welfcap	*D.welfpub*	*D.welfgdp*
L. GDPCAP	0.05211***	0.00122	−0.00011
	(3.27)	(1.11)	(0.48)
L. OUTPUT GAP	0.77892	0.17950	0.08657***
	(0.57)	(1.05)	(2.62)
L. AGE65	34.91644***	−0.64500	0.25328
	(2.93)	(0.52)	(1.15)
L. GOVERNMENT	3.30616***	0.29859	0.09605***
	(3.51)	(1.58)	(3.98)
D. GOVERNMENT	10.25068***	−0.19431	0.26407***
	(8.82)	(0.65)	(10.49)
L. EX.RATE	0.00711	−0.00247	−0.00025
	(0.92)	(1.51)	(1.20)
D. EX.RATE	0.02375	0.00195	0.00070
	(1.36)	(0.50)	(1.54)
L. TRADE	0.30471	0.08263	−0.00682
	(0.46)	(0.93)	(0.53)
D. TRADE	−1.49695***	−0.07054	−0.03724***
	(4.11)	(1.46)	(4.33)
L. CAPITAL	0.26930	0.15297**	0.00451
	(0.59)	(2.42)	(0.65)
D. CAPITAL	0.09342	0.08632	−0.00035
	(0.22)	(1.59)	(0.04)
L. DEMOCRACY	−9.04328	0.84851	−0.13790
	(1.08)	(0.56)	(0.70)
D. DEMOCRACY	−16.96761	1.02375	−0.51814
	(1.11)	(0.39)	(1.56)
L. POPULAR	−17.68635**	0.66099	−0.09809
	(2.38)	(0.62)	(0.57)
D. POPULAR	3.57339	3.59734***	0.11421
	(0.50)	(3.05)	(0.63)
Dec-70	−0.01802**	−0.00223*	−0.00013
	(2.22)	(1.82)	(0.89)
Dec-80	−0.01474	−0.00193	−0.00015
	(1.54)	(1.54)	(0.82)
L. trade*capital	−34.78092***	−6.32199***	−0.86041***
	(2.86)	(3.31)	(2.95)

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TABLE 2 *(cont.)*

D. trade*capital	−34.16169***	−4.79902***	−0.90888***
	(4.02)	(3.20)	(4.40)
Lagged DV	−0.30021***	−0.64301***	−0.28627***
	(3.24)	(4.89)	(4.32)
Constant	−546.11518***	23.32253	0.32960
	(3.21)	(1.62)	(0.11)
R-squared	0.4724	0.3757	0.4613
Prob>Wald Chi2	0.0000	0.0013	0.0000
Observations	273	273	273

* significant at 10%; ** significant at 5%; *** significant at 1%; panel-corrected z-statistics in parentheses

[a] See Table 1 (fn. a).

as is shown by the uninteracted coefficient of trade levels in Table 2, this is not statistically significant. In the real world, scores on the capital index ranged from 20 to almost 90, and the effects of trade turn sharply and significantly negative as the capital account is opened. At a level of 90 points, which most countries approximated by the late 1990s, a 10 percent increase in trade would predict a decline of $41 in per capita spending. We obtained comparable results for each of the other two specifications of spending; as capital controls are removed, trade has an increasingly negative and significant effect on spending.

We defer until the next section an extended discussion of the political variables but note here that, with our aggregate specifications of social spending, the effects are generally weak and if anything go against expectations. Against much of the literature on the OECD countries, popularly based governments actually appear to spend significantly less per capita than alternative governments, although in the short term they do spend a greater share of the budget.

Democratic regimes also have no predictable impact on aggregate social expenditures; and we found no robust interaction effects between democracy and any of the other independent variables used in the model. In contrast to Brown and Hunter,[46] for example, once globalization variables were entered into the model, democracy appeared no more responsive than dictatorships to demographic and political pressures and no less resistant to downturns in the economy. It is quite possible, of course, that these null and counterintuitive results are the product of measurement error. We will argue in the following section,

[46] Brown and Hunter (fn. 16).

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however, that we can get a better understanding of the effects of the political variables if we examine their impact on different *types* of social spending.

IV. Welfare Spending Disaggregated: Pensions and Health and Education

Expenditures on social security, health, and education have typically been combined in analytical overviews of social spending in Latin America.[47] There are reasons to believe, however, that they may be influenced by different political logics. In this section, therefore, we disaggregate social spending and reexamine separately the effects of the globalization and political variables on social security transfers and on human capital expenditures on health and education.

There are several reasons why social security expenditures might be especially susceptible to the efficiency pressures of trade integration and therefore less likely to be defended by democratic regimes. First, most of the spending in this category goes to pension payments. As these are financed in part through payroll taxes that have a direct and transparent impact on the cost of labor, one might expect business groups to press especially hard for holding them down.

Even more important, pension benefits are typically the most regressive component of social spending.[48] The social security category does include antipoverty programs and targeted assistance to the poor, but pension payments themselves flow mainly to the middle class and to workers in the formal sector, while the costs of financing large pension-fund deficits are socialized through general taxation or inflation.[49] Thus, with the possible exception of a few very comprehensive pension systems such as those in Uruguay or Costa Rica, cutbacks in the pension component of social security spending may be less likely to generate widespread popular protest than has been the case in many European countries.[50]

Conceivably, the political constraints and opportunities are different in the case of human capital expenditures on health and education. Although health insurance is also sometimes a component of the wage bill, these expenditures generally have a smaller direct impact on labor

[47] See ECLAC (fn. 24); and Brown and Hunter (fn. 16).
[48] See ECLAC (fn. 24); and Stallings and Peres (fn. 8).
[49] Mesa-Lago (fn. 19).
[50] For a discussion of the European cases, see Gøsta Esping-Andersen, *The Three Worlds of Welfare Capitalism* (Princeton: Princeton University Press, 1990); and idem (fn. 21). See also Pierson (fn. 12).

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costs; and from the point of view of employers, they may have more substantial payoffs as investments in human capital.

There is also a greater likelihood of strong political opposition to cutbacks in these areas. Despite inequities and the severe inadequacy of social service delivery systems, human capital expenditures typically reach a larger segment of the population than pensions.[51] In-depth country studies of Argentina, Brazil, Chile, and Colombia, for example, show that spending on health and education constitutes about 75 percent of the total social expenditures received by families in the lowest income quintile and has a positive impact on the overall distribution of income.[52]

In short, as Latin American economies become more integrated into global markets, incumbent governments may have stronger political incentives to protect health and education expenditures than to protect social security. In fact, the simple correlation (Pearson's $r = -0.52$) between these measures as a percentage of the budget does imply a rather sharp trade-off. Particularly in an era of hard budget constraints, governments appear to be under considerable pressure to set priorities.

Tables 3 and 4 show how expenditures in social security and in health and education are affected by the variables used in the general model. In each specification of spending, the cumulative impact of these variables remains relatively strong; the R^2 ranges from .30 to 45, and the estimates for both the globalization and political variables are very much in line with our expectations.

Let us look first at the globalization variables in the two tables. Table 3 shows that the impact of the trade variable that we saw in the general model works primarily through its effects on pensions and transfers. Trade openings have a uniformly negative effect on social security spending, in both the short run and the long run. The coefficient for capital openness is significant only with long-term per capita expenditure, but all but one of the others are negative as well. And although we do not display the results, the effects of the interaction are identical with those found in the general model: capital account liberalization compounds the effect of trade.

The models for health and education expenditures (Table 4) show a very different picture. First, although five of the six signs for trade continue to be negative, none reaches even a 0.1 level of significance. Thus, we cannot reject the hypothesis that trade has no effects on this cate-

[51] See ECLAC (fn. 24).
[52] See Mostajo (fn. 15).

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TABLE 3
DETERMINANTS OF SOCIAL SECURITY SPENDING IN 14 LATIN AMERICAN COUNTRIES[a] (1973–97)

	D.sscap	*D.sspub*	*D.ssgdp*
L. GDPCAP	0.02908***	−0.00088	−0.00014
	(2.62)	(1.03)	(0.68)
L. OUTPUT GAP	1.24428	0.37265**	0.08093**
	(0.98)	(2.30)	(2.48)
L. AGE65	21.19791**	0.17069	0.24539
	(2.06)	(0.17)	(1.24)
L. GOVERNMENT	2.83378***	0.33744**	0.08555***
	(4.02)	(2.52)	(4.82)
D. GOVERNMENT	7.48964***	0.13620	0.17224***
	(7.52)	(0.61)	(7.65)
L. EX.RATE	0.00372	−0.00270**	−0.00020
	(0.54)	(2.30)	(1.13)
D. EX.RATE	0.02053	0.00426	0.00061
	(1.34)	(1.46)	(1.59)
L. TRADE	−0.90592***	−0.06344**	−0.01908***
	(3.16)	(2.09)	(3.05)
D. TRADE	−1.36058***	−0.12208***	−0.03658***
	(3.97)	(3.11)	(4.32)
L. CAPITAL	−0.37124*	0.01552	−0.00295
	(1.73)	(0.63)	(0.66)
D. CAPITAL	−0.02702	−0.00227	−0.00873
	(0.08)	(0.05)	(1.16)
L. DEMOCRACY	−17.09170**	−1.44737	−0.37006*
	(2.00)	(1.18)	(1.82)
D. DEMOCRACY	−20.87167	−1.02922	−0.61415**
	(1.42)	(0.52)	(2.05)
L. POPULAR	−9.62654	2.79490***	0.06050
	(1.36)	(2.71)	(0.35)
D. POPULAR	16.63647**	3.96490***	0.55272**
	(1.99)	(3.46)	(2.52)
Dec-70	−42.70732***	−6.68163***	−1.04712***
	(3.85)	(4.68)	(4.03)
Dec-80	−37.53505***	−4.80568***	−0.97314***
	(4.85)	(4.16)	(5.71)
Lagged DV	−0.26595***	−0.56006***	−0.33047***
	(3.03)	(5.42)	(4.68)
Constant	−228.76248*	32.88103***	1.50973
	(1.83)	(3.00)	(0.59)
R-squared	0.3754	0.3723	0.3848
Prob>Wald Chi2	0.0000	0.0000	0.0000
Observations	284	284	284

* significant at 10%; ** significant at 5%; *** significant at 1%; panel-corrected z-statistics in parentheses

[a]See Table 1 (fn. a).

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TABLE 4
DETERMINANTS OF HEALTH AND EDUCATION EXPENDITURES IN 14 LATIN AMERICAN COUNTRIES[a] (1973–97)

	D.humcap	*D.humpub*	*D.humgdp*
L. GDPCAP	0.02482***	0.00081	−0.00007
	(5.26)	(1.59)	(0.81)
L. OUTPUT GAP	−0.53871	−0.13127	−0.00671
	(0.75)	(1.07)	(0.27)
L. DEPENDENTS	−0.32490	−0.05597	−0.00592
	(1.63)	(1.35)	(0.86)
L. GOVERNMENT	0.72537*	−0.04439	0.04446***
	(1.86)	(0.52)	(3.13)
D. GOVERNMENT	2.74380***	−0.29154**	0.09617***
	(4.75)	(2.03)	(5.23)
L. EX.RATE	0.00110	0.00178	0.00003
	(0.44)	(1.56)	(0.26)
D. EX.RATE	0.00350	−0.00085	0.00028
	(0.67)	(0.36)	(1.03)
L. TRADE	−0.18819	−0.01729	−0.00443
	(1.48)	(0.65)	(0.96)
D. TRADE	−0.17520	0.00936	−0.00533
	(1.02)	(0.25)	(0.81)
L. CAPITAL	−0.05147	0.04568**	0.00583*
	(0.50)	(2.38)	(1.85)
D. CAPITAL	0.26331	0.09700***	0.00802
	(1.35)	(2.62)	(1.34)
L. DEMOCRACY	7.71009**	1.85676**	0.22071*
	(2.02)	(2.01)	(1.77)
D. DEMOCRACY	2.67727	2.49207*	0.01699
	(0.53)	(1.83)	(0.10)
L. POPULAR	−4.87457	−1.40306*	−0.16366
	(1.32)	(1.68)	(1.14)
D. POPULAR	−12.89505**	−1.75167	−0.44433**
	(2.32)	(1.58)	(2.18)
Dec-70	2.74960	1.76608	0.15769
	(0.48)	(1.54)	(0.76)
Dec-80	−4.28160	0.97024	−0.07972
	(1.09)	(1.20)	(0.56)
Lagged DV	−0.50365***	−0.47954***	−0.49991***
	(6.15)	(6.03)	(5.40)
Constant	−128.27304***	−1.97991	0.45990
	(3.95)	(0.38)	(0.60)
R-squared	0.4609	0.3048	0.3961
Prob>Wald Chi2	0.0000	0.0000	0.0000
Observations	284	284	284

* significant at 10%; ** significant at 5%; *** significant at 1%; panel-corrected z-statistics in parentheses

[a]See Table 1 (fn. a).

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gory of expenditure. Even more interesting, both capital openness coefficients show positive and significant effects on budget shares, arguably the most direct indication of government spending priorities; they are also significant for spending as a percentage of GDP over the long run. These results are considerably less stable than those for trade in alternative specifications of the model, so it is important to be cautious in the weight we attach to them. Nevertheless, they do provide support for the compensation argument.

The implications of the political variables are perhaps even more interesting. Unlike in the general model, a substantial number of the coefficients for democracy and partisanship are significant in Tables 3 and 4, and they appear to highlight the importance of the trade-offs we have noted between social security and human capital expenditures.

Table 3 shows that popularly based governments have a significant short-term effect in all three specifications of the model. Over the long term the budgetary priority attached to social security is also likely to be higher during years when popularly based governments are in power. When all else is held constant, such governments can be predicted to raise budget shares by about 5 percent. As argued, such spending priorities tend to be consistent with the preferences of the trade union base of these leaders and/or the ideological preferences of their party supporters.

Conversely, democracies appear to have a negative effect on such programs. Although the impact on budgetary allocations is negligible, there are statistically significant effects on social security spending per capita and as a share of GDP in both the short and the long term. Since we initially hypothesized that elected governments might respond disproportionately to middle- and working-class voters who support such programs, this came as something of a surprise. We must add, moreover, that results are—unlike trade—sensitive to the specification of the model and did not uniformly reach standard levels of significance with other estimation techniques. Even so, the results are in line with what we know about the inequitable features of such programs, and they indicate that, at a minimum, democracies can cut expenditures without much concern for an electoral backlash.

This speculation receives additional support when we turn to the results for health and education spending shown in Table 4. The impact of popularly based governments is negative, now across all three specifications of social spending. This suggests that popularly based governments are inclined to squeeze human capital expenditures, possibly to protect pension spending. By contrast, transitions to democracy

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(D-democracy) lead quickly to an increase in budget allocations for human capital. Over the long term democratic regimes produce spending increases in all three specifications of the model. Expenditures rise by about $15 per capita (about 7 percent over the regional average) and by .45 of GDP (about 18 percent over the regional average). Democratic governments increase the fiscal share of health and education by about 4 percent (16 percent over the regional average).

Of course, we cannot be entirely sure of the causal mechanisms behind this result; democratic regimes may also be responding, for example, to pressures from health workers and teachers unions, which tend to be among the strongest components of organized labor. Some pressure may also be coming from enlightened international and domestic capitalists interested in increasing labor skills. When we contrast the impact of democracy on human capital expenditures with its impact on social security, however, it is plausible to infer that the preferences of a mass electorate are also playing a role.

V. Conclusions

Like most statistical studies, our findings leave open a variety of questions, many of which can be answered only by more qualitative research methods. In the first section and elsewhere in the article, we have discussed a number of explanations for the relations we have found between globalization, political pressures, and social expenditures. In many instances, however, we cannot be sure about which of a number of causal mechanisms actually affect outcomes. We have discussed this issue at some length in the case of the short- and long-term effects of trade liberalization; we cannot be certain about the extent to which cutbacks in aggregate social spending and in social security reflect producer pressures, the initiatives of government decision makers, or the indifference or even opposition of sectors that are excluded from benefits. Similar questions can be raised about other findings as well, for example, the positive effects of capital openness on investments on health and education spending or the inclination of popularly based governments to reduce these expenditures.

To answer such questions requires a closer analysis of the organization of social service systems, of who has benefited from them in the past, and of who stands to gain or lose from changes in the future. As noted briefly in the introduction, some of the most vexing challenges of second-phase welfare reforms have less to do with the amount of financing than with the way financing is allocated and with how delivery

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systems are organized. Such issues are often best analyzed through case studies and small-N comparisons that provide close examinations of the politics of the budget process and of bargaining over the design and implementation of social services.

If our analysis cannot definitively uncover the causal mechanisms that underlie the statistical findings, however, it does provide a frame of reference that might orient future research. Three sets of conclusions are of particular importance. The first concerns the contending arguments about the effects of trade integration on the welfare state: the overwhelming weight of evidence favors the efficiency over the compensation hypothesis. Although we cannot be entirely sure about why this is so, we can infer from our findings that trade integration does change power resources in ways that lead to a reduction in pensions and other transfers, the components of social spending that provide the most direct protections from vulnerability to market forces. Even in the case of health and education expenditures, moreover, there is no evidence that expansion of trade encourages states to enlarge the size of their welfare commitments. Indeed, if anything, trade openings have a negative impact on these components of social spending as well.

Integration into global capital markets has a more ambiguous effect. On the one hand, it does appear to encourage increases (or discourage decreases) in spending on health and education, possibly as a way to upgrade the quality of the labor force available to foreign investors. On the other hand, capital account liberalization also compounds the negative effects of trade openings on social security expenditures. Presumably this is because it increases the leverage of producers of traded goods who seek to contain the cost of labor. As their economies become more closely linked to capital markets, they can make more credible threats to liquidate their assets and shift them elsewhere.

A second important conclusion is that it is important to distinguish between the different types of social spending. The distinctions used in this study follow those conventionally used in studies of the UN Economic Commission on Latin America and the Caribbean. Social security transfers are, as just noted, most relevant to the efficiency and compensation hypotheses, since they both add to the wage bill and offer the most direct protection against market forces. Health and education expenditures arguably involve longer-term investments in human capital and are likely to have a greater long-term impact on the distribution of income.

The point that is clearly indicated in this study, however, is that these categories of social spending are influenced by very different sets of po-

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litical and economic factors. The good news is that the pronounced constraints that globalization appears to place on social security transfers do not extend to spending on health and education. Possibly because the health and education sectors encompass a wider set of stakeholders, decisions on spending in these areas appear to reflect a very different political logic, much more connected to electoral competition and political participation. Although qualitative case studies of social sector reforms are more concerned with restructuring than with expenditures, it should be noted that they also consistently show distinctions between the politics of pension reform and the politically more difficult challenges related to the restructuring of social service sectors.[53]

We have not attempted to explore the politics of social sector reform in this paper. To maintain the clarity of our presentation, moreover, we have also left for later efforts to analyze differences between expenditures in health and those in education. Given our findings so far, however, an exploration of these questions would be a logical next step for further research.

Our third set of conclusions concerns the way spending is affected by domestic political variables. We show that democratic regimes and popularly based governments do have effects that are independent of the impact of globalization. These effects, however, depend a great deal on the constellation of interests affected by different types of social programs, and they work in very different directions.

Like social democratic governments in Western Europe, popularly based governments in Latin America are an important force for the protection or extension of welfare transfer programs. In Western Europe, however, programs directed toward the large, unionized working class have generally contributed to a reduction of inequality, whereas in Latin America's segmented labor markets benefits for formal sector workers can reinforce the gap between them and those in agriculture and the informal sector. It is perhaps for this reason that we see democracy working systematically to hold down social security spending.

Contrary to the findings of Brown and Hunter,[54] we did not find that democracy had a strong impact on *overall* levels of spending, but this is only because these measure aggregate programs with quite dif-

[53] See, for example, Joan M. Nelson, "Social Costs, Social-Sector Reforms, and Politics in Post-Communist Transformations," in Joan M. Nelson, Charles Tilly, and Lee Walker, eds., *Transforming Post-Communist Political Economies* (Washington, D.C.: National Academy Press, 1997).

[54] Brown and Hunter (fn. 16).

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ferent social effects. In keeping with their argument that "regimes matter," democratically elected governments did protect expenditures on health and education. Although more nuanced examinations of constitutional design and institutional arrangements can undoubtedly tell us more about such effects, our findings suggest that Latin American democracies do generally support demands for more progressive forms of social spending.

Appendix 1: Description and Source of Variables

WELFCAP. Social expenditures per capita. Social expenditures include public expenditures in health care, education, and social security programs; measured in 1995 constant U.S. dollars. Source: Created with data from the IMF, *Government Finance Statistics* (various issues).

WELFPUB. Social expenditures as a percentage of central government spending.

WELFGDP. Social expenditures as a percentage of GDP.

GOVERNMENT. Central government spending as percentage of GDP.

GDPCAP. GDP per capita in 1995 constant U.S. dollars. Source: World Bank, *World Development Indicators 2000* .

EXCHANGE RATE. Real exchange rate. Nominal exchange* (U.S.CPI index/National CPI index). Sources: For the nominal exchange rate, *World Development Indicators*; formula for the real exchange rate comes from Krugman (fn. 30).

OUTPUT GAP. Difference between real GDP in local currency units at constant prices and the underlying growth trends, as a percentage of the trend. A Hodrick-Prescott filter (H-P) is used to estimate the underlying growth trend. The H-P filter uses long-run moving average to detrend the output series. The method is used frequently by financial and policy institutions such as the IMF and the OECD. It minimizes the sum squared of deviations of actual output around its trend, subject to a constraint on the variation of the growth rate of trend output. It calculates the trend as the solution to the following minimization problem.

$$\underset{\{y_t^T\}}{Min} \sum_{t=1}^{T}[(y_t - y_t^T)^2 + \lambda \left[\left(y_{t+1}^T - y_t^T\right) - \left(y_t^T - y_{t-1}^T\right)\right]^2]$$

where, for each period, the trend values y_t^T minimize the above equation for a given value of the smoothing parameter λ.

DEPENDENTS. Age dependency ratio. Measures the number of dependents over the working-age population. The age dependency ratio is

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calculated as the ratio of dependents—the population under age 15 and above age 65—to the working-age population—those aged 15–64. Source: *World Development Indicators*.

AGE65. Percentage of the population over 65. Source: *World Development Indicators*.

TRADE. Imports plus exports as a percentage of GDP. Source: *World Development Indicators*.

CAPITAL. Measures the degree of freedom from government restrictions on capital mobility. The values have been normalized from 0 and 1, with 1 being perfectly free from distortion (no legal restrictions to the flow of capital). We have multiplied the index by 100 to facilitate interpretation in terms of percentages. Source: Morley, Machado, and Pettinato (fn. 26).

DEMOCRACY. Dummy variable with a value of 1 in democratic years and 0 in nondemocratic years. A country is considered democratic if it reaches a score of 6 or higher after subtracting AUTOC from DEMOC in the Polity III data set. Source: Jaggers and Gurr (fn. 28). The values for 1995, 1996, and 1997 come from an update of this study called *Policy IV*.

POPULAR. Dummy variable coded 1 for years in which a popularly oriented president was in office and 0 otherwise. Source: See Table 5.

TABLE 5
CODINGS FOR POPULARLY BASED PRESIDENTS[a]
(1973–97)

Country	*Presidents*	*Period*
Argentina	Isabel M. de Perón (Peronist Party)	1974–75[b]
	Carlos S. Menem (Peronist Party)	1990–97
Bolivia	Siles Suazo (MNR)	1983–84
	Paz Estensoro (MNR)	1985–89
	Paz Zamora (MIR)	1990–93
	Gonzalo Sánchez de Lozada (MNR)	1994–97
Chile	Salvador Allende (Socialist Party of Chile)	1973
	Patricio Aylwin/Eduardo Frei (Concertación)	1990–97
Costa Rica	José Figueres (PLN)	1973
	Daniel Oduber (PLN)	1974–77
	Luis Alberto Monge (PLN)	1982–85
	Óscar Arias (PLN)	1986–89
	José María Figueres (PLN)	1994–97
Dominican Republic	Antonio Guzmán Fernández (PRD)	1979–82
	Salvador Jorge Blanco (PRD)	1983–86
	Peña Gómez (PRD)	1997

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TABLE 5 *(cont.)*

Country	*Presidents*	*Period*
Mexico	Luís Echeverría (PRI)	1973–76
	José López Portillo (PRI)	1977–82
	Miguel de la Madrid (PRI)	1983–88
	Carlos Salinas (PRI)	1989–94
	Ernesto Zedillo (PRI)	1995–97
Ecuador	Rodriguez Lara/[c]	1973–76
	Poveda/Duran/Franco[c]	1976–79
	Rodrigo Borja (Democratic Left)	1989–92
Peru	Velasco Alvarado[c]	1973–75
	Alan García (APRA)	1985–90
Venezuela	Carlos Andrés Pérez (AD)	1974–78
	Jaime Lusinchi (AD)	1984–88
	Carlos Andrés Pérez (AD)	1989–92

[a] If a popularly based president takes office between January and June, that year is coded as "popular." If the president takes office between July and December, however, the year is coded "not popular." A similar criterion is applied about presidents leaving office or being deposed. If a president leaves office before June, the year is coded as "not popular" (unless the next president is also popularly based); if the president leaves office between July and December, the year is coded as "popular."

[b] Juan Perón took office in October 1973. The previous president from the Peronist Party, Hector Cámpora, had been sworn in in May and resigned in July. Thus, it did not seem that they were in power long enough to introduce any significant policy changes. We have therefore coded 1973 as "not popular." Isabel Perón became president in July 1974, replacing her husband Juan Perón, who had just died. She was deposed by military coup in March 1976. We have therefore coded as "popular" only 1974 and 1975.

[c] This is a popularly based military president or junta.

APPENDIX 2

The error correction model is given by

$$\Delta Y_{i,t} = \alpha + \Delta X_{i,t-1}\beta_k + \phi\,(Y_{i,t-1} - X_{i,t-1}\Upsilon) + \varepsilon_{i,t} \qquad (1)$$

where, in our case, $Y_{i,t}$ is social expenditures in country *i* during year *t*, Δ is the first-difference operator, X is a vector of independent variables, and $\varepsilon_{i,t}$ is a white noise error term. The model describes a short-term equilibrium relationship given by $\Delta Y_{i,t} = \alpha + \Delta X_{i,t-1}\beta_k + \varepsilon_{i,t}$ and a term $\phi\,(Y_{i,t-1} - X_{i,t-1}\Upsilon)$, which measures the deviation from this short-term equilibrium relationship. (Note that $\varepsilon_{i,t-1} = (Y_{i,t-1} - X_{i,t-1}\Upsilon)$). Equation 1 shows that, first, a one-off change in $X_{i,t-1}$ produces a contemporary change in $Y_{i,t}$. This short-term effect is determined by the k-dimen-

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sional vector of regressors β_k. Furthermore, when the impact of $X_{i,t-1}$ on $Y_{i,t}$ throws the model off its long-run equilibrium (given by the cointegrating vector $Y^*_{i,t-1} = X^*_{i,t-1}\Upsilon$, where the "*" indicates equilibrium), the discrepancy or "error" $(Y_{i,t-1} - X_{i,t-1}\Upsilon)$ is corrected at a yearly rate of ϕ.

One way to show in a more intuitive way how to interpret the different short-term and long-term coefficients is to transform equation 1 through a simple mathematical operation: let β_j be defined as $-(\phi\Upsilon)$, where both parameters ϕ and Υ come from equation 1, then it follows that $\Upsilon = \beta_j / -\phi$. Equation 1 can therefore be rewritten as

$$\Delta Y_{i,t} = \alpha + Y_{i,t-1}\phi + \Delta X_{i,t-1}\beta_k + X_{i,t-1}\beta_j + \varepsilon_{i,t} \qquad (2)$$

Equation 2 is then estimated through OLS. The interpretation of the coefficients is then as follows. The regression coefficient for an independent level variable is a measure of the long-run equilibrium relationship between a vector of cointegrated independent variables (that is, sharing the same long-run trend) and the dependent variable. As noted above, the long-run equilibrium relationship is given by $Y^*_{i,t-1} = X^*_{i,t-1}\Upsilon$. The parameter Υ (which measures this long-run equilibrium relationship) is not directly observable from equation 2 but can be found by dividing β_j by $-\phi$ (see above).

The importance of the short-term effects $\Delta X_{i,t-1}$ depends, however, on the size of β_k and on how long the effects of changes in $X_{i,t-1}$ persist over time. A one-off change in $X_{i,t-1}$ produces an immediate (contemporary) change in $Y_{i,t}$ that is measured by β_k. If at time t there is a change in $X_{i,t}$ in the opposite direction to the change in $X_{i,t-1}$, then there are no more effects. But if the change in $X_{i,t-1}$ is sustained, then the impact will continue in subsequent periods and can be measured by $\Delta X_{i,t-1}(1 + \phi)^t$, where t is the number of periods after the initial change. Thus, for example, three years after the initial change $\Delta X_{i,t-1}$, the effect will be $\Delta X_{i,t-1}(1 + \phi)^3$. Since $0 < \phi < -1$, the smaller the value of ϕ, the longer the sustained changes in X will persist over time.

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COMPETITIVE CORRUPTION
Factional Conflict and Political Malfeasance in Postwar Italian Christian Democracy

By MIRIAM A. GOLDEN and ERIC C. C. CHANG*

THIS article is motivated by three central theoretical puzzles regarding systemic political corruption in democratic settings. First, political corruption may involve high-level politicians and in extreme cases may involve even an entire political class. As such, it is clearly not a problem of inadequate or ineffective monitoring by politicians of the bureaucrats they supervise, as is the case with the more commonly studied phenomenon of bureaucratic corruption.[1] Second, systemic political corruption in a democratic context occurs despite a wealth of legal regulations intended to prevent it. The cases of persistent, high-level corruption of which we are aware—recent revelations feature Germany and France, as well as Italy—do not occur in settings that exhibit substantially less transparency or other judicial or constitutional characteristics that obviously account for their unusual outcomes. Finally, Schumpeterian competition between political elites ought to prevent large-scale, persistent political corruption from taking root in democratic polities in the first place, precisely because honest partisan competitors can always offer their services to voters and ought to be preferred over their dishonest counterparts. The very existence of wide-

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[1] For the distinction between political and bureaucratic corruption, see Susan Rose-Ackerman, *Corruption: A Study in Political Economy* (New York: Academic Press, 1978).

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spread, persistent political corruption in a competitive electoral system constitutes a puzzle.

This leads us to ask when and why political competition may fail to inhibit rent seeking on the part of elected officials—specifically, the illegal receipt of monies in the form of bribes and kickbacks that constitutes the core of political corruption.[2] Persson, Tabellini, and Trebbi hypothesize that although electoral competition does not eliminate rent seeking, some types of electoral rules make elected officials more accountable than others and hence less prone to corrupt behaviors.[3] They contend that party lists lead to more corruption than occurs in political systems in which individuals compete against each other for public office, because electoral systems characterized by the latter procedure allow voters to control individual representatives. Others subscribe to same view.[4]

Students of countries that actually use proportional representation and party lists tend to believe the reverse, however. According to these scholars, electoral systems that provide incentives for politicians to develop personal reputations generate less accountability, higher rents, and potentially more corruption than those encouraging the development of party reputations, perhaps because parties wield instruments that can discipline their members.[5] This view stands in direct contrast to the arguments advanced by Persson, Tabellini, and Trebbi.

Our study contributes to this debate by systematically testing the hypothesis that competition among individual candidates from the same party is significantly associated with political corruption. While this hypothesis arises naturally from the theory advanced by Persson,

[2] Although political corruption encompasses a broader set of activities, the receipt of funds in exchange for favors is especially important, both because of the potentially distortionary economic effects of these activities and because they are almost always illegal. When elected officials persistently engage in illegal behavior, troubling consequences arise for the democratic order.

[3] Torsten Persson, Guido Tabellini, and Francesco Trebbi, "Electoral Rules and Corruption" (Manuscript, Institute for International Economic Studies, Stockholm University, November 2000).

[4] For instance, Paul Mitchell, "Voters and Their Representatives: Electoral Institutions and Delegation in Parliamentary Democracies," *European Journal of Political Research* 37 (May 2000).

[5] Those whose work supports this view include Barry Ames, "Electoral Strategy under Open-List Proportional Representation," *American Journal of Political Science* 39 (May 1995); Bruce Cain, John Ferejohn, and Morris Fiorina, *The Personal Vote: Constituency Service and Electoral Independence* (Cambridge: Harvard University Press, 1987); John M. Carey and Matthew Soberg Shugart, "Incentives to Cultivate a Personal Vote: A Rank Ordering of Electoral Formulas," *Electoral Studies* 14 (December 1995); Morris P. Fiorina and Roger Noll, "Voters, Bureaucrats and Legislators: A Rational Choice Perspective on the Growth of Bureaucracy," *Journal of Public Economics* 9 (April 1978); idem, "Voters, Legislators and Bureaucracy: Institutional Design in the Public Sector," *American Economic Review Papers and Proceedings* 68 (May 1978); Barbara Geddes and Artur Ribeiro Neto, "Institutional Sources of Corruption in Brazil," *Third World Quarterly* 13, no. 4 (1992); and David J. Samuels, "Incentives to Cultivate a Party Vote in Candidate-Centric Electoral Systems: Evidence from Brazil," *Comparative Political Studies* 32 (June 1999).

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Tabellini, and Trebbi, they do not test it. Instead, they compare the extent of reported political corruption in countries using majoritarian electoral systems with its extent in those using proportional representation (PR). Theories of what Cain, Ferejohn, and Fiorina have called "the personal vote"[6] suggest however that the more useful distinction lies between systems in which parties control candidate selection—closed-list PR—and those in which they do not, including majoritarian systems with primary elections and other two-ballot systems, single nontransferable vote (SNTV) systems, and open-list PR. This theory classes together electoral systems using SNTV (such as Japan), those using open-list PR (such as Italy), and those using two-ballot systems in which candidates of the same party compete against each other on the first ballot (such as France and the United States).

The "personal vote" literature typically focuses on how politicians provide constituency service in order to gain individual recognition from voters, although the circumstances under which constituency service also involves patronage and, in the extreme, outright corruption have not been identified. Plausibly, however, incomplete or blocked partisan competition for public office is a contributing factor, along with a legal environment that easily criminalizes campaign fund-raising efforts or contributions.[7]

Cross-national testing of these arguments requires building an appropriate data set—a difficult task, given the complexity of ranking electoral systems according to their degree of campaign and candidate personalism.[8] In the meantime, we draw on data from Italy's postwar legislative elections, specifically examining the association between charges of corruption and intraparty competition in the country's major governing party, Italian Christian Democracy (DC), during the postwar era. While there are some disadvantages to studying the impact of electoral rules on political corruption within a single national setting in which electoral rules themselves do not undergo modification, these limitations are fewer than may initially seem to be the case.[9] We take advantage of the rules in operation in Italy from after World War II until the 1994 elections (when the electoral system was modified) to investigate the impact of intraparty competition—or the search for votes by individual candidates within the DC—on charges of malfeasance

[6] Cain, Ferejohn, and Fiorina (fn. 5).

[7] See Miriam A. Golden, "The Effects of the Personal Vote on Political Patronage, Bureaucracy and Legislation in Postwar Italy" (Manuscript, University of California at Los Angeles, May 2001).

[8] See Carey and Shugart (fn. 5).

[9] Note as well that electoral systems are so rarely modified that studying the effects of change across time within countries generates only a handful of cases.

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against DC members of parliament. To anticipate our findings, we show that political corruption is significantly associated with increases in intraparty competition. Although the determinants of political corruption are many, the present study, along with that of Persson, Tabellini, and Trebbi,[10] are the only existing systematic analyses we know of that examine the impact of electoral institutions on political corruption.

The Italian case was selected for four main reasons. First, Italy represents an extreme case of systemic political corruption in an established, democratic setting. Rich (and therefore democratic) countries tend to enjoy relatively low levels of corruption, at least if surveys of international executives such as those conducted by Transparency International (TI) are to be believed.[11] Per capita gross domestic product does a good job explaining perceived corruption among a sample of some fifty-odd countries analyzed by Treisman, for example.[12] Italy, as Figure 1 shows, remains the most pronounced exception. It is the country for which wealth is least effective as the single explanatory variable of perceived levels of corruption, as revealed by the fact that it is farthest off the regression line depicting a linear relationship between per capita gross domestic product and perceived levels of corruption. In fact, the studentized residual of Italy in the regression shown in Figure 1 is 3.1543, substantially above the normal threshold of 2. If we set Italy as a case indicator to distinguish it from other countries, the Italian coefficient in the expanded regression model is significantly different from 0 at less than the 0.01 level. These results suggest that Italy is even exceptional among outliers. In the mid-1990s Italian corruption levels were far higher than the country's relatively wealthy status would have predicted. Working with such an extreme case makes what is usually almost impossible to study a feasible area of empirical investigation. Not only are the traits and mechanisms of corruption more visible in such a setting, but there is much more of it to observe.

Second, an implication of the finding reported above regarding Italy's position in Figure 1—that the country is so far off the regression line—is that unusual, nonrandom influences may be at work. While it

[10] See fn. 3.

[11] The TI index fails to distinguish political from bureaucratic corruption, and the latter is undoubtedly more common than the former. We have no reason to believe that Italy's location in the index would change, however, even if the index were confined to political corruption for the period through the mid-1990s; if anything, the Italian case would probably become relatively even more extreme. After the mid-1990s the TI index for Italy is probably capturing bureaucratic corruption, earlier political corruption, or some combination. See below (fn. 87) for further discussion.

[12] Daniel Treisman, "The Causes of Corruption: A Cross-National Study," *Journal of Public Economics* 76 (June 2000); see also Alberto Ades and Rafael Di Tello, "Rents, Competition, and Corruption," *American Economic Review* 89 (September 1999).

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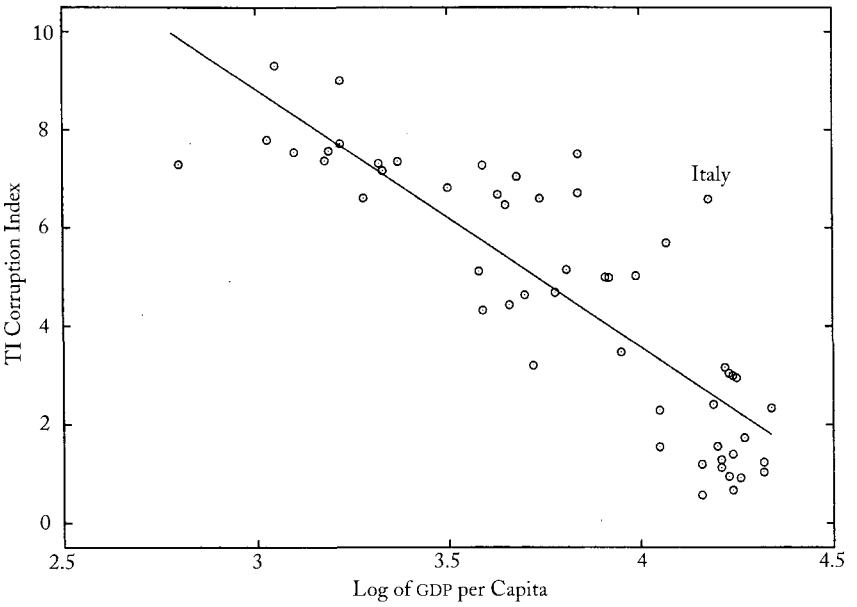

FIGURE 1
THE RELATIONSHIP BETWEEN WEALTH AND PERCEIVED CORRUPTION IN VARIOUS COUNTRIES, C. 1990

SOURCE: Treisman (fn. 12).

[a] The Transparency International index has been inverted so that higher scores indicate greater perceived corruption.

would therefore be unwise to construct a general theory of political corruption in democratic settings based exclusively or even predominantly on information gleaned from the Italian case, any such theory would have to reckon with this case—even if only to explain why it is so deviant. Anything we can reliably learn about the causes of corruption in postwar Italy will therefore ultimately contribute to theory building.

Third, Italy offers a unique and heretofore underexploited source of data on political corruption: requests by magistrates for authorization to proceed with an investigation against a member of parliament, or what are abbreviated in Italian as RAP (*le richieste di autorizzazione a procedere)*. In essence, these are requests to suspend the parliamentary immunity of a legislator. These requests are made by investigating magistrates seeking to proceed with legal charges against an MP (whether a senator or a member of the Chamber of Deputies). Between 1948 and

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1993, the Italian judiciary made 4,770 requests to parliament for authorization to proceed with such charges.[13]

Finally, the dramatic revelations of extremely widespread corruption among the Italian political class with the "Clean Hands" investigations that began in 1992 generated a rich literature describing the processes of Italian political corruption. While it was no secret even prior to 1992 that political parties in Italy had been engaged in extensive corruption (as a handful of studies had documented[14] but as was also well known even in the absence of systematic research), after 1992 numerous aspects of Italian political corruption were scrutinized, generating a rich and detailed literature in both Italian[15] and English.[16] In addition, the revelations of corruption generated firsthand testimonials by some of the most knowledgeable participants.[17] As a result, Italy offers the single most important source of information about the nature, extent, and workings of widespread political corruption in a modern democratic context.

Despite the wealth of available literature, little is known with confidence about the causes of political corruption—in Italy or elsewhere.

[13] Franco Cazzola and Massimo Morisi, "Magistratura e classe politica: due punti di osservazione specifici per una ricerca empirica," *Sociologia del Diritto* 22, no. 1 (1995), 101–2.

[14] Most importantly, Franco Cazzola, *Della corruzione: fisiologia e patologia di un sistema politico* (Bologna: Il Mulino, 1988); and Donatella della Porta, *Lo scambio occulto: casi di corruzione politica in Italia* (Bologna: Il Mulino, 1992).

[15] Among others, Franco Cazzola, *L'Italia del pizzo: fenomenologia della tangente quotidiana* (Turin: Einaudi, 1992); Alessandro Pizzorno, "Introduzione: la corruzione nel sistema politico," in della Porta (fn. 14); Franco Cazzola, "Storia e anatomia della corruzione in Italia," *Il Politico* 13 (April 1993); Luca Ricolfi, *L'ultimo Parlamento: sulla fine della prima Repubblica* (Rome: La Nuova Italia Scientifica, 1993); Donatella della Porta and Alberto Vannucci, *Corruzione politica* e *amministrazione pubblica: risorse, meccanismi, attori* (Bologna: Il Mulino, 1994); Mauro Magatti, *Corruzione politica* e *società italiana* (Bologna: Il Mulino, 1996); and Alberto Vannucci, *Il mercato della corruzione: i meccanismi dello scambio occulto in Italia* (Milan: Società Aperta, 1997).

[16] Including Donatella della Porta and Alberto Vannucci, "Politics, the Mafia, and the Market for Corrupt Exchange," in Carol Mershon and Gianfranco Pasquino, eds., *Italian Politics: Ending the First Republic*, vol. 9 of *Italian Politics: A Review* (Boulder, Colo.: Westview Press, 1995); Sarah Waters, "'Tangentopoli' and the Emergence of a New Political Order in Italy," *West European Politics* 17 (January 1994); David Moss, "Patronage Revisited: The Dynamics of Information and Reputation," *Journal of Modern Italian Studies* 1 (Fall 1995); Donatella della Porta and Alessandro Pizzorno, "The Business Politicians: Reflections from a Study of Political Corruption," *Journal of Law and Society* 23 (March 1996); David Hine, "Political Corruption in Italy," in Walter Little and Eduardo Posada-Carbó, eds., *Political Corruption in Europe and Latin America* (Bassingstoke: Macmillan, 1996); Martin Rhodes, "Financing Political Parties in Italy: A Case of Systemic Corruption," *West European Politics* 20 (January 1997); Stanton H. Burnett and Luca Mantovani, *The Italian Guillotine: Operation Clean Hands and the Overthrow of Italy's First Republic* (Lanham, Md.: Rowman and Littlefield, 1998); and Donatella della Porta and Alberto Vannucci, *Corrupt Exchanges: Actors, Resources, and Mechanisms of Political Corruption* (New York: Aldine de Gruyter, 1999).

[17] Gherardo Colombo, *Il vizio della memoria* (Milan: Feltrinelli, 1996); Piercamillo Davigo, *La giubba del re. Intervista sulla corruzione*, ed. Davide Pinardi (Rome: Laterza, 1998); and Antonio Di Pietro, *Intervista su Tangentopoli*, interview with Giovanni Valentini (Rome: Laterza, 2000).

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The Italian literature has focused mainly on systematizing the mechanisms and practices of widespread political corruption and on identifying and classifying the types of behaviors and actors involved in corrupt exchanges. This has generated an unusually rich literature, often based on exhaustive primary research, detailing the practices of political corruption in postwar Italy. However, systematic hypothesis testing of the *causes* of widespread political corruption has been uncommon. The literature on developing countries, although older and often extensive, tended to be relatively unsystematic in its forms of analysis. Only recently have scholars begun to produce systematic work on the causes of corruption.[18] This scholarship has been cross-national in scope, and hence the hypotheses examined have necessarily been limited by data availability. Thus, for example, the absence of data over time has precluded examining temporal change.

This article, constrained by the limitations in the data that are available, offers a partial but systematic causal account of political corruption over the course of the postwar era in Italy—partial in the sense that it explicitly tests the impact only of political sources of systemic corruption. (Other possible types of causes, broadly categorized, include economic and cultural.) To compensate to some degree, we supplement our causal investigation with an exploration of the temporal geography of political corruption in postwar Italy that allows a preliminary assessment of the prima facie plausibility of a rival cultural perspective. Despite current data limitation, our analysis generates some robust findings.

We demonstrate that the unusually high levels of corruption observed in postwar Italy were in part an outgrowth of specific features of the open-list proportional representation electoral system in use there, features that promoted conflict between candidates from the same party. To document this, we assemble a database measuring intraparty competition in the country's dominant political party, Christian Democracy. We analyze this in conjunction with a data set that we assembled on charges of malfeasance against DC members of parliament. The analysis shows that beginning in the early 1970s, a significant statistical relationship existed between intraparty conflict in the ruling Christian Democratic Party and the extent of suspected malfeasance among DC members of Italy's Chamber of Deputies. Our analysis also finds that electoral competition with other parties shows no significant statistical impact on alleged malfeasance by DC legislators. We then examine the

[18] Especially Ades and Di Tello (fn. 12); Persson, Tabellini, and Trebbi (fn. 3); and Treisman (fn. 12).

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geography of political corruption over time in Italy, in order to assess whether corruption spread via a contagion process or whether it arose out of characteristics of the political system. The findings fail to corroborate a geographic or ancillary cultural perspective, again leading us to endorse an institutional interpretation. We offer evidence that political corruption among Christian Democratic legislators appears to have become systematic about 1974, just when a law regarding the public financing of political parties was adopted. This suggests that legislation designed to limit discretionary financing of political parties may well have amplified the degree of corruption arising out of the electoral system. Corruption is operationalized in our analysis as alleged criminal wrongdoing. Although actual corruption, usually defined as the use of public office for personal or partisan gain, was undoubtedly more common than the proxy measure of suspected malfeasance employed in this study, data on actual corruption are, of course, not available.

I. Institutional Origins of Systemic Political Corruption

We examine three hypotheses in this section. The first is that political corruption is in part an outgrowth of *intraparty* competition, which in turn is partially a function of specific characteristics of the electoral system. The second is that *interparty* competition may also significantly affect political corruption. The third, which derives from our results on the first two, is that a legal change in the mid-1970s over the public financing of Italian parties may have magnified political corruption. We now detail the basic logic of these hypotheses.

Systems of proportional representation, in which multiple representatives are elected from each district in proportion to the share of votes cast for their various parties, may use closed or open lists for ordering candidates. An open-list system forces candidates of the same party to compete against each other, because even though districts are multimember, the actual persons elected are decided not by the order established on the party list (which effectively allows parties to discipline their own candidates) but by the number of votes received by individual candidates. Katz has plausibly contended that this procedure pits party members against each other,[19] creating powerful incentives toward internal party disunity. A similar syndrome is associated with systems (such as that used in Japan until 1994) of the single, nontransferable

[19] Richard S. Katz, "Intraparty Preference Voting," in Bernard Grofman and Arend Lijphart, eds., *Electoral Laws and Their Political Consequences* (New York: Agathon Press, 1986).

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vote. Both SNTV and open-list PR typically (but not inevitably)[20] result in significant factionalism within parties. And factions, of course, need funds to compete effectively against one another.

Typically, however, national party organizations have little interest in funding specific factions and instead direct resources mainly toward political competition with other parties. In Italy, for instance, public funding for political campaigns, established in 1974, directed funds to national party organizations, not to individual candidates.[21] This essentially leaves individual candidates and factions on their own to raise the necessary campaign funds. As Cox and Thies show using Japanese data,[22] intraparty electoral competition is expensive, so incentives are strong to raise money.[23] Illicit campaign contributions are one result.

Anecdotal evidence suggests that such an interpretation of the electoral system may be useful for understanding the unusually high level of political corruption in postwar Italy. While it is impossible to quantify the extent to which political graft was used for intraparty competition, there is evidence that it was apparently extensive.[24] One observer noted in the mid-1970s that "few Christian Democrats win seats without running personalized campaigns that are both extensive and costly."[25] Whole political careers were made on the basis of illicit monies. Della Porta and Vannucci, in their detailed study of the results of judicial investigations into corruption in Italy, argue that illicit contributions to the Christian Democrats—for there were also many to other parties, especially the Italian Socialist Party (PSI)—were aimed not at the DC as a whole but at particular factions within it.[26] This suggests that it is worth making a more systematic investigation of the relationship between intraparty competition and political corruption.

At the same time that intraparty competition may engender political

[20] Samuels (fn. 5).

[21] Linda Bull, "Public Money, Political Parties, and Corruption: The Italian Case," *Italian Politics and Society: The Review of the Conference Group on Italian Politics and Society* 48 (Autumn 1997). The putative impact of this law is analyzed below.

[22] Gary W. Cox and Michael F. Thies, "The Cost of Intraparty Competition: The Single, Non-Transferable Vote and Money Politics in Japan," *Comparative Political Studies* 31 (June 1998).

[23] Documented in Eric C. C. Chang, "The Institutional Roots of Political Corruption in Postwar Japan" (Manuscript, Department of Political Science, University of California at Los Angeles, January 2000); and Gary W. Cox and Michael F. Thies, "How Much Does Money Matter: 'Buying' Votes in Japan, 1967–1990," *Comparative Political Studies* 33 (February 2000).

[24] Della Porta and Vannucci (fn. 15), 225–26.

[25] Donald Wertman, "The Italian Electoral Process: The Elections of June 1976," in Howard R. Penniman, ed., *Italy at the Polls: The Parliamentary Elections of* 1976 (Washington, D.C.: American Enterprise Institute for Public Policy Research, 1977), 76. While Wertman nowhere specifically mentions illegality as a means of raising campaign funds, he repeatedly stresses that "little, in fact, is known about the sources of factional funds" (p. 77).

[26] Della Porta and Vannucci (fn. 15), 236.

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corruption, interparty competition may also augment it. If fear of seat loss drives attempts to raise campaign funds—attempts that may easily slide into illegality—perhaps it does not matter whether the potential loss occurs because of rivalry within the same party or because of competition with candidates from other parties. To examine whether this is the case, we include measures of interparty competition in the multiple regressions analyzing the political causes of suspected malfeasance among DC members of parliament.

Finally, we test the impact on the rise of corruption of the 1974 law regarding the public financing of Italy's political parties. Whereas parties had previously been entirely self-financing, after 1974—in what proved to be an ironic attempt to "clean up" campaign financing—parties received public funding for their campaigns. At the same time, the law prohibited public companies from making contributions to parties and required parties to make annual public disclosure of their sources of funds.[27] The law both criminalized much preexisting behavior and eventually—with the Clean Hands investigations of the 1990s—provided the legal grounds for prosecution of many politicians.

Measures and Data

The Dependent Variable: Political Corruption

For obvious reasons, it is difficult to measure political corruption. By its nature, corrupt activity tends to be secret, and those involved have incentives to keep it that way. Studies of the phenomenon have relied on three different types of data in democratic contexts: expert or informed opinion, press reports, and/or legal proceedings or outcomes.

An example of the first—indeed, probably the most prominent example—is the comparative index of corruption made available by Transparency International on the basis of opinions of businesspeople.[28] These data are useful for getting a broad-gauged idea of the extent of corruption in different countries. Data limitations make it impossible to use this source for any substantial period of time or to analyze the causes of corruption within countries.

Press reports can also be utilized for assessing corruption, although, like all sources, their reliability is often unclear. They have been used in some empirical investigations.[29] However, even where widespread cor-

[27] For a description of the law in English, see Wertman (fn. 25), 77.

[28] Useful discussions include Thomas D. Lancaster and Gabriella R. Montinola, "Toward a Methodology for the Comparative Study of Political Corruption," *Crime, Law and Social Change* 27, nos. 3–4 (1997); and Johann G. Lambsdorff, "Corruption in Comparative Perspective," in Arvind K. Jain, ed., *Economics of Corruption* (Boston: Kluwer, 1998).

[29] For instance, Cazzola (fn. 14), chap. 3.

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ruption exists, the press may not expose it; or it may expose trivial incidents of graft but not have access to the information that would allow exposés of major networks of criminal behavior among politicians. Finally, gathering and coding media reports is typically a laborious and analytically imprecise process.

Court proceedings or outcomes, finally, can be used to measure corruption. Most of the existing empirical work on corruption in Italy uses legal records of various types, including the extremely useful studies by della Porta, della Porta and Vannucci, and Vannucci,[30] which draw heavily on transcripts of the trials of those charged with political corruption. Our research strategy complements theirs.

In this study, we make use of a related legal source of information: charges of malfeasance against members of parliament. The reason for the frequency of such requests in postwar Italy lies with the unusually high guarantees of parliamentary immunity that protected parliamentarians. Until 1993 parliamentarians were protected from prosecution except when an absolute majority of the members of the house in which they served explicitly agreed to do otherwise.[31] Not surprisingly, in the years after World War II, most requests were not allowed to proceed and instead were denied or simply allowed to lapse for lack of response. One study finds that only 19 percent of RAP were approved during the years between 1948 and 1987, and in some legislatures (mainly in the early postwar era) the percentage was in the single digits.[32] Regardless of the outcome, however, the data on the requests themselves are a useful source of information on suspected malfeasance by members of parliament.

Parliament maintains its own records of all requests for authorization to proceed. These include the date of the request, the charges, the location of the magistrate's office making the request (that is, the name of the city), the name of the MP being charged, and the eventual response by parliament (approval to proceed, rejection, or failure to act). Some research using these data has already been conducted.[33] Cazzola generously provided his data set for use in the present analysis. It includes his coding of the house in which the representative served

[30] See della Porta (fn. 14); della Porta and Vannucci (fn. 15); della Porta and Vannucci (fn. 16); and Vannucci (fn. 15).

[31] As of November 1993 the judiciary was automatically allowed to proceed unless an absolute majority voted against the request. This change in procedure greatly facilitated the Clean Hands investigations in the 1990s.

[32] Cazzola (fn. 14), 113, table IV.1.

[33] Cazzola (fn. 14); Cazzola (fn. 15); Cazzola and Morisi (fn. 13); Franco Cazzola and Massimo Morisi, *La mutua diffidenza: il reciproco controllo tra magistrati* e *politici nella prima Repubblica* (Milan: Feltrinelli, 1996); and Ricolfi (fn. 15).

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TABLE 1
CHARGES OF MALFEASANCE AGAINST DC MEMBERS OF THE TWO HOUSES OF PARLIAMENT, FIRST TEN LEGISLATURES[a]

Legislature	*Deputies Charged/Total (%)*	*Senators Charged/Total (%)*	*Difference %*
I (1948–53)[b]	34/306 (11)	12/150 (8)	3*
II (1953–58)	24/262 (9)	5/111 (4)	5*
III (1958–63)	35/273 (13)	10/121 (8)	5*
IV (1963–68)	28/260 (11)	6/134 (4)	6***
V (1968–72)	22/265 (8)	7/137 (5)	3*
VI (1972–76)	51/265 (19)	16/136 (12)	7**
VII (1976-79)	26/262 (10)	13/136 (10)	0
VIII (1979–83)	38/262 (14)	20/139 (14)	0
IX (1983-87)	46/226 (20)	11/121 (9)	11***
X (1987–92)[c]	42/234 (18)	13/127 (10)	8**

*significant at the 0.1 level or better; **significant at the 0.05 level or better; ***significant at the 0.01 level or better

SOURCES: Data courtesy of Franco Cazzola; originally provided by the Italian parliament.

[a]Data include all charges of wrongdoing.

[b]Results for the I Legislature are marginally significant.

[c]Data for the X Legislature are incomplete.

(Chamber or Senate), the MP's partisan affiliation, and his or her electoral district, as well as the various categories of data that we have already enumerated as available in the original Italian parliamentary records. We supplemented this data set, which ends in 1987, with data provided by a parliamentary office on charges of malfeasance against members of the Chamber of Deputies of the X and XI Legislatures. We coded the latter data for each MP's electoral district. In using Cazzola's coding distinguishing senators from deputies, we decided to drop the former from the analysis, both because senators were elected from single-member districts rather than by open-list PR and because we did not have data on charges of wrongdoing against members of the Italian Senate for the X and XI Legislatures.

Our guiding hypothesis about the effects of open-list PR suggests that members of the Italian Senate should have been less exposed to the kinds of intraparty pressures that generated the need for illegal campaign funds, precisely because the Senate was not elected by open-list PR. To confirm this, Table 1 shows data comparing the proportion of DC deputies charged and the proportion of DC senators charged in each of the first ten postwar legislatures. (Note that the proportion is measured as the total number of DC representatives charged divided by the total number of DC representatives elected to each chamber. The XI

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Legislature is excluded because Cazzola's data set extends only to 1987.) We tested whether the difference between the two proportions was significantly different from 0 for each legislature. The results reported in the table show that the proportion of DC deputies charged is significantly higher than the proportion of DC senators charged in eight out of ten legislative periods. As the hypothesis linking the electoral system to intraparty competition and then to malfeasance predicts, Christian Democratic senators tended to be less likely to be charged with malfeasance than were Christian Democratic deputies.[34] We interpret the fact that the differences in the proportions (although statistically significant by conventional standards) are often substantively not very large as indicating the contamination of *all* DC party politics by the incentives arising from the electoral system found in the Chamber of Deputies during this period. Senators were significantly less exposed to incentives to engage in political wrongdoing, but given the frequent crossover of politicians from one house to the other, they were far from immune.

Readers will note from the data reported in Table 1 that a relatively small number of elected officials in the DC were investigated for any types of wrongdoing in the years between 1948 and 1987, when this data set ends. A wave of charges occurred in the period following 1987, and these subsequent charges (until the end of the XI Legislature in 1994) are included in the data set used for the remainder of this study. That data set is therefore considerably larger than the one in the analysis reported in Table 1.

Two major issues affect the validity of our measure of political corruption. First, requests to lift parliamentary immunity do not distinguish between allegations of corruption and allegations of other sorts of malfeasance. In principle, it should be possible to separate out charges of suspected corruption from other crimes, since the parliamentary records identify the legal statutes brought to bear in levying charges against the suspect.[35] In practice, however, this is more difficult than might appear. Doing so requires interpreting the specific statutes cited. These run the gamut from murder to speeding. Only one study of

[34] We tested this hypothesis only for Christian Democratic legislators because our full data set did not separate out "opinion" crimes from other types of charges for members of other parties; for a discussion of types of charges, see pp. 600–601.

[35] Cazzola (fn. 14) distinguishes corruption from other types of suspected malfeasance, but he does not detail his coding procedures, making evaluation of them impossible. Hence, we chose not to use his classification. His study classed 40 percent of the RAP against DC parliamentarians between 1948 and 1987 as involving political corruption.

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which we are aware[36] provides a classification scheme that distinguishes political corruption from other types of suspected malfeasance. However, because this classification has not been systematically evaluated in the literature, it is unclear whether it can legitimately serve as a standard.

Furthermore, the high threshold for the suspension of parliamentary immunity in Italy introduces conceptual problems into any supposedly neat distinction between allegations of "corruption" and allegations of other types of wrongdoing. Precisely because it was so difficult to strip parliamentarians of their immunity and because parliamentarians were aware that they were likely to be protected from prosecution by their colleagues, almost any crime could have involved some element of abuse of office. Unlike ordinary citizens, deputies and senators were largely protected from the legal consequences of breaking the law—surely a subtle encouragement to wrongdoing. As a result, parliamentarians, knowing they would not be stripped of their parliamentary immunity for minor acts—or even, in many cases, for major ones—were little deterred from committing offenses. Nor is it surprising, therefore, that Italian commentators refer frequently to the extent to which the governing parties came to exhibit an "arrogance of power." Any classification that interprets "corruption" simply as pocketing public monies or accepting illegal campaign contributions is likely to be excessively narrow in such a context.

Our solution was to drop observations that we knew conclusively were unrelated to political corruption, namely, charges involving libel, slander, defamation, and other "opinion" crimes, including slander against the state and slander of the public administration.[37] This left in our data set all charges of wrongdoing conceivably related to abuse of public office and political corruption. The analysis presented in the remainder of this paper uses this reduced data set.

The second problem of validity is potentially more serious, in that we are less able to respond effectively to it. The measure of corruption that we use makes it impossible to distinguish suspected malfeasance on the part of legislators from the behavior of the judiciary, which is charged with investigating and prosecuting. While this problem is severe in principle, in practice it constitutes a contaminating measurement problem only if the judiciary's behavior generates *systematic* bias in the RAP. If there is no systematic bias, then as in any statistical analysis, we simply assume that the variable is "noisy." As we detail below, we

[36] Ricolfi (fn. 15), 151–64.

[37] This procedure was suggested to us by Davide Petrini. The exact statutes dropped from the analysis were numbers 341–42 and numbers 594–99.

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believe that of the various possible types of systematic bias that could characterize the data, only one—possible systematic underinvestigation of DC politicians between 1948 and the mid-1970s—is likely to constitute a genuine problem. To anticipate, however, our statistically significant results do not bear on the period prior to the early 1970s, so the problem of potential data bias is simply not relevant to our findings. We now detail our full line of argument about possible biases in the data.

The kinds of systematic biases we could observe include the systematic *underinvestigation* of alleged wrongdoing by deputies in the governing parties because of criminal or partisan associations on the part of the investigators; or systematic *overinvestigation* (that is, deliberate attempts to smear the reputations of deputies in the governing parties) because of the partisan associations of the judiciary or factional conflicts among politicians themselves, leading some to denounce their rivals secretly to the judiciary.

Systematic underinvestigation because of criminal associations among the judiciary appears to have been a relatively minor phenomenon. There are judges who are known associates of the Mafia, but this does not appear widespread, despite reports, such as that found in Gambetta,[38] of instances in which judges repeatedly pronounced dubious rulings on Mafia suspects but were not removed or transferred.

In any politically competitive situation, accusations of corruption may come, perhaps without adequate justification, from political opponents. Factions within the governing parties may systematically pass potentially incriminating information on to the judiciary in order to disable rivals from other factions in the same party. But whereas the political use of potentially incriminating information seems relatively common in some other countries, the literature we have reviewed does not suggest that behavior along these lines occurred in Italy, where factional and partisan affiliation served instead to signal trustworthiness among the dishonest. Denunciations of others within the same party or among the ruling parties would have undermined the networks that had been established to bind the corrupt together, allowing predictable divisions of the spoils; moreover, such denunciations would have provided political ammunition to the main opposition party, the Italian Communist Party (PCI) , whose increasing ability to attract the votes of citizens dissatisfied with the system of DC rule came to pose a distinct political threat by the 1970s. The PCI's relative lack of involvement in

[38] Diego Gambetta, *The Sicilian Mafia: The Business of Private Protection* (Cambridge: Harvard University Press, 1993), 6–7.

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political corruption, its emphasis on "good government" and "the moral question," and its growing electoral appeal made it politically all the more important that the communists (and the public generally) not be allowed access to incriminating information about members of the ruling parties.[39] There was little incentive for members of the ruling cartel to turn to the judiciary with accusations against other members.[40]

There have been instances in which the judiciary has been known to issue requests to proceed for alleged "crimes" that were largely partisan-political in nature. In the early postwar era, not surprisingly, when most members of the judiciary were holdovers from the fascist era, numerous RAP were issued against communist MPs for alleged crimes involving "sabotage" against the Italian state and other purported efforts to undermine the constitution.[41] Some have contended—quite plausibly—that underinvestigation of political corruption by members of the governing parties routinely occurred in the 1950s and 1960s.[42] Likewise, others have argued[43] that the RAP issued in the course of the Clean Hands investigations of the 1990s were largely politically motivated by an ideological judiciary consisting overwhelmingly of zealots of the left. Our reading of the evidence is that the facts do not substantiate the latter interpretation.[44]

Of possible instances of under- and overinvestigation of deputies due to judicial bias, only the systematic underinvestigation of the DC between 1948 and the mid-1970s is therefore empirically relevant. There is, given data now available, no way to know. If this occurred, then the results reported below for the first half of the legislatures that we ana-

[39] The charge that by failing to denounce corruption in the ruling parties the PCI thereby colluded in it has been made by Silvia Colazingari and Susan Rose-Ackerman, "Corruption in a Paternalistic Democracy: Lessons from Italy for Latin America," *Political Science Quarterly* 113 (November 1998). It also underlies Alessandro Pizzorno's characterization of Italy's postwar political system as a "consociational" arrangement between government and opposition elites; see Pizzorno, "Le difficoltà del consociativismo," in idem, *Le radici della politica assoluta e altri saggi* (Milan: Feltrinelli, 1993); and idem (fn. 15). This view fails to acknowledge the difficulties that must have been experienced by the PCI and such little independent press as existed in Italy in obtaining credible information about political corruption, given a system in which those involved in it systematically covered up for each other.

[40] The same is not true of accusations of libel, slander, and defamation of character, which apparently often came from political rivals within the same party. By omitting cases involving such charges, we remove this potential source of bias.

[41] Since the present analysis is confined to the DC and omits charges of insult of a public official (statute no. 341) and insult of the state (statute no. 342), this does not pose a problem for us.

[42] Della Porta and Vannucci (fn. 16, 1999), 141–44.

[43] Burnett and Mantovani (fn. 16).

[44] For instance, the magistrates involved in the Milan pool were themselves affiliated with a variety of political parties or with no party at all; some had left-wing political histories but some did not, and so on. Even Burnett and Mantovani (fn. 16), although arguing this position, do not offer persuasive and thorough documentation supporting the contention that the Milanese judiciary was politically compact.

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lyze may be biased. But if they are biased, it is in favor of the null hypothesis—which, until the early 1970s, is precisely the result we report. That is, we do not claim that systematic relationships exist between the independent variables we analyze and charges of malfeasance against DC deputies in the years before the early 1970s. If we did, possible bias in the data would reduce our confidence in such a claim. Substantively, this means there may have been more political corruption in Italy in the 1950s and 1960s than we observe using these data, and indeed that analyses drawing on the RAP are unlikely to generate systematic statistical results for this period. It may be difficult ever to know the causes of political corruption in Italy over the entire postwar era, given that for much of the period, so much presumed corruption remained uninvestigated. But this in no way invalidates the results of our analysis for the period starting in 1972.[45]

THE INDEPENDENT VARIABLES: INTRAPARTY AND INTERPARTY COMPETITION

In this section we discuss our measure of intraparty competition and then the ways we operationalize interparty competition.

As we have already argued, Italy's open-list electoral system forced candidates of the same party to compete with each other. As recent work on Brazil has noted, even in electoral systems that encourage "the personal vote," specific political parties may overcome the incentives toward personalism, especially if they are without access to the resources offered by control of government itself.[46] This is precisely the case for the Italian Communist Party, a well-disciplined and centralized organization denied national power throughout the period. For reasons that lie beyond the scope of the present article, the party successfully overcame the incentives of the electoral system toward intraparty competition and factionalism, instructing its electors in the distribution of their preference votes[47] where it did not simply discourage preference voting altogether. Hence, in the 1970s, for instance, the PCI received the smallest number of preference votes of any of Italy's five largest parties.[48] The Christian Democrats, by contrast, found themselves dividing

[45] Ultimately, only additional research into the postwar Italian judiciary will allow us to distinguish more fully our measure of suspected political wrongdoing from judicial independence. One possible measure of potential judiciary autonomy is the extent of resources available to different judicial offices throughout the country over time, but formidable data collection problems would have to be confronted.

[46] Samuels (fn. 5).

[47] Wertman (fn. 25), 75.

[48] See the estimates in Richard S. Katz, "Preference Voting in Italy: Votes of Opinion, Belonging, or Exchange," *Comparative Political Studies* 18 (July 1985), 233, table 1.

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into factions by the mid-1950s, as politicians competed for internal resources and sought to build patronage machines capable of lining up preference votes.[49] By the 1960s the DC's factions were formally organized, each with its own headquarters, news agency, regularly scheduled meetings, and lists for party office.[50] The party adopted PR for internal party elections beginning in 1971,[51] so that factions enjoyed formal representation in DC governing bodies according to their relative strength. By the 1980s factional strength on the DC's National Council was calculated down to hundredths of percentages in the allocation of cabinet positions.[52] The National Council was in turn elected from within the party, and competition for both party and government positions was regulated largely by counting "the amount of preference votes obtained in party and government elections."[53] Amassing preference votes was hence critical not only to winning office but also to controlling resources within the national party organization.

DC factions were generally not ideologically distinct (although they can be arrayed ideologically) but instead were largely organized around the patronage machines controlled by individual politicians, mainly national parliamentarians. These factions constituted the cartel that governed Italy from 1948 until the so-called opening to the left in 1963, when the PSI first entered a coalition government with the DC. This enlarged the cartel to include the Socialists, most notably; minor cartel members during the postwar era also included the Italian Liberal Party (PLI), the Italian Republican Party (PRI), and the Italian Social Democratic Party (PSDI). There is now considerable documentation that all governing parties were systematically involved in kickbacks and corruption, that agreements existed among them regulating the proportion of the spoils each would receive, that corrupt exchanges involved large numbers of national politicians, and that they covered up for each other (in part by voting against lifting parliamentary immunity of those charged with corruption). The complex networks across parties, between nationally organized factions and local political machines, from electoral officials to bureaucrats in the public administration—members

[49] Gianfranco Pasquino, "Le radici del frazionismo e il voto di preferenza," *Rivista Italiana di Scienza Politica* 2 (August 1972).

[50] Alan S. Zuckerman, *The Politics of Faction: Christian Democratic Rule in Italy* (New Haven: Yale University Press, 1979), 111.

[51] Giovanni Sartori, "Proporzionalismo, frazionismo e crisi dei partiti," *Rivista Italiana di Scienza Politica* 1 (December 1971).

[52] Reported by Carol Mershon, *The Costs of Competition* (Stanford, Calif.: Stanford University Press, forthcoming.)

[53] Zuckerman (fn. 50), 67.

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of each of them involved in a gigantic web of corruption, kickbacks, and bribes—is beyond the scope of the present analysis.

This said, it is important to note that although local politicians apparently were to some extent free to determine the division of the bribes that would occur locally, the Clean Hands investigations revealed a system of corrupt exchanges in which national party leaders—the members of parliament studied here—were active participants and possibly the initial instigators. We study members of parliament mainly because of the availability of systematic data, but this choice is justified by the fact that the trail of evidence uncovered in the investigations that took place in the 1990s clearly led to them, revealing a system of corruption that was ultimately national in scope. Political corruption in postwar Italy involved many persons beyond those studied here, but Christian Democratic deputies were critical figures in the networks of corrupt exchanges that developed.

Although measures of intraparty conflict in the DC at the level of electoral districts are not readily available, a good proxy can be constructed. This consists of summing the number of preference votes given to DC candidates for the Chamber of Deputies in each electoral district and deflating the sum by the total number of list votes received by the party in the same election and same district, for each of the country's thirty-one (until the 1958 election, thirty) electoral districts that used PR.[54]

Until the 1992 elections Italy had an extremely pure system of PR.[55] (In 1994 Italy adopted a mixed system of PR and majoritarianism, in which the latter predominated.) Under the PR system, the parties listed candidates for each electoral district, each of which sent an average of twenty representatives to the Chamber. Voters marked their ballots with their party of choice (known as list votes). At the same time, voters were allowed to indicate up to four "preferences" for individual candidates listed by the party for which they voted, although they could choose to exercise fewer or even no preferences.[56] Only about a third of all possible preference votes were normally cast.[57] A 1991 referendum

[54] Italy's smallest region, the Valle d'Aosta, was, because of its size, a single-member district that used a plurality system. Because intraparty competition did not occur there, the district is not included in the analysis reported below.

[55] For descriptions in English, see Samuel H. Barnes, *Representation in Italy: Institutionalized Tradition and Electoral Choice* (Chicago: University of Chicago Press, 1977), chap. 3; and Wertman (fn. 25).

[56] Districts electing sixteen or more representatives allowed a maximum of four preference votes to each elector; those electing fewer allowed three; reported in Barnes (fn. 55), 36.

[57] Richard S. Katz and Lucio Bardi, "Preference Voting and Turnover in Italian Parliamentary Elections," *American Journal of Political Science* 24 (February 1980), 99.

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resulted in the reduction of preference votes to only one,[58] so the analysis of the 1992 parliamentary elections reported below uses the new measure. The actual individuals seated in parliament were those who had received the most preference votes. Getting a large number of preference votes was thus essential to attaining or retaining office—indeed, within the DC, the single most important cause of turnover in the two parliamentary elections held in the 1970s was intraparty defeat[59]—and also important for acquiring or retaining influence within the party.

We restrict the present analysis to the DC, for three reasons. First, the party was factionalized for a much longer period than other Italian parties, thereby permitting a longer valid time period for analysis. Second, familiarity with Italian history suggests that postwar political corruption originated in the DC. Third, we currently have data on preference votes for DC candidates only.[60] In principle, the analysis undertaken here could be extended to include all the governing parties, and it would be especially interesting to enlarge the analysis to include the Italian Socialist Party.

The data on preference votes received by individual candidates, as well as district-level electoral data, are available for every postwar election in a series of volumes commonly known as *La Navicella*, after the publisher.[61] We constructed a data set that included the name of each DC candidate for each district in each election, the number of preference votes received by each, and whether the candidate was subsequently seated in the Chamber.[62] We also included the number of list votes received by the DC in each election and each district,[63] as well as the elec-

[58] For an analysis, see Gianfranco Pasquino, "La preferenza unica: reazioni e opportunità," in Gianfranco Pasquino, ed., *Votare un solo candidato: le consequenze politiche della preferenza unica* (Bologna: Il Mulino, 1993).

[59] Katz and Bardi (fn. 57).

[60] Figures on preference votes have to be assembled and electronically input by hand, candidate by candidate, district by district, and election by election.

[61] The volumes are available starting with La Navicella, *I Deputati e Senatari del Prima Parlamento Repubblicano* (Rome: La Navicella, 1948).

[62] There were exceptions to the rule that candidates receiving the most preference votes received parliamentary seats. Occasionally, a candidate died before taking his seat. More common was the phenomenon of standing in multiple districts, since Italian law permitted parliamentary candidates to stand in as many as three districts, as well as to run for both the Senate and the Chamber simultaneously. In these cases, candidates who received enough preference votes in different districts to win more than one parliamentary seat were required to select the district they would represent as well as the house in which they would be seated. Hence, there were instances in which a candidate who received enough preference votes to win a Chamber seat did not in fact subsequently take that seat, instead opting for another.

[63] Where necessary, we supplemented the information available in La Navicella with data from Gianfranco Pasquino, ed., *1945–1996: archivio della politica in Italia* (Rome: Laterza Multimedia, CD Rom, 1996).

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toral quota required in each election and each district to win a Chamber seat and the total number of deputies elected from each district.

It seems plausible that the increases in political competition and the seat loss that affected DC members of parliament with the rise of the left over the postwar era may have fueled the growth of corruption among Christian Democrats, as MPs sought to raise funds to protect themselves from losing office. In 1948 the DC controlled 48.5 percent of seats in the Chamber of Deputies; in 1963 this fell drastically to 38.3 percent. Another sharp decline occurred in 1983, when the DC's share fell to 32.9 percent. Thus over time both the party and many individual DC politicians came to be increasingly exposed to interparty competition.

We constructed three separate measures of interparty competition: (1) the change in the proportion of *list votes* received by the DC in each election, (2) the change in the proportion of *seats* held by the DC in each election, and, finally, (3) a measure of *marginality,* constructed as the ratio between the number of votes received by the lowest-ranked DC candidate who won a seat (that is, the winning DC candidate who garnered the smallest number of preference votes among party contenders) and the electoral quota for the district (that is, the number of votes required to win a seat). The last measure captures how "safe" the most marginal DC seat is.[64]

We constructed each of these three measures using three different types of lags: the change at time t over $t_{-1;}$ the change at time t_{-1} over $t_{-2;}$ and the change at time t over t_{+1}. The reason for using different lags was that, although we believed that illegal activities among political candidates would be greatest in the period just prior to national elections, we had no theory of when candidates would feel most threatened by other parties. Various scenarios seemed plausible: perhaps vote or seat loss in one election would engender more illegal fund-raising in the next period; perhaps the threat of vote or seat loss would engender contemporaneous illegal fund-raising; or perhaps, finally, illegal fund-raising occurred in anticipation of electoral losses.

Analysis and Results

To examine whether intra- and interparty conflict were significant determinants of suspected political corruption of Christian Democratic legislators, we set up a series of regressions of the following form:

[64] As far as we know, there is no standard measure of marginality in PR settings. This one seemed to us a reasonable approximation. Alternatives that occurred to us—such as the difference between the number of preference votes received by the highest- and second-highest-ranking DC candidates—captured *intraparty* competition more than *interparty* competition.

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$$SUSCOR = \beta_{0it} + \beta_{1it}\text{INTRA} + \beta_{2it}\text{INTER} + \sum_{t=1}^{10} \gamma_t Year + \varepsilon_{it} \quad (1)$$

where

— i is the electoral district
— t is the year of the legislative election
— SUSCOR is suspected corruption on the part of DC deputies, measured as the number of nonopinion crime RAP lodged against DC MPs over the life of the subsequent legislature divided by the number of DC MPs (or the proportion of DC deputies charged)[65]
— INTRA is intraparty competition, measured as the total number of preference votes received by Christian Democratic candidates divided by the total number of list votes received by that party in the same district
— INTER is interparty competition, measured as
the share of DC votes (model 1) or
the share of DC seats (model 2) or
the number of preference votes received by the winning DC candidate with the lowest number of preference votes over the electoral quota (model 3)[66]
— YEAR is the election year dummy variable to pick up any variance unaccounted for by the main predictors and
— ε is an error term

Parliamentary elections were held in 1948, 1953, 1958, 1963, 1968, 1972, 1976, 1979, 1983, 1987, and 1992. Table 2 reports the results of pooled regressions that were run using each of the three measures of interparty competition detailed above. We report panel-corrected standard errors in order to guard against potential problems of heteroskedasticity across districts and contemporaneous correlation of errors across years.[67] The table shows the results only for those regressions using $t - t_{-1}$, but results with alternate lags were similar.[68]

First, notice in Table 2 that the probability of a large-F distribution is low enough to allow one to reject the null hypothesis that there is no relationship between the explanatory variables and the dependent variable at the 0.001 level in all three models. These models all reveal strong and significant relationships between our independent and dependent variables. Even more striking is that the coefficients for INTRA are positive and significant at the 0.01 level in all three models, whereas

[65] Since multiple sets of charges may be lodged against the same individual deputy, where a single MP was charged more than once during the life of a single legislature, we dropped the multiple observations, thereby capping the number of RAP against a single MP at 1 for any legislature.

[66] Each measure of the variable INTER was constructed using each of the three lags discussed above, generating nine separate regressions.

[67] Nathaniel Beck and Jonathan N. Katz, "What To Do (and Not To Do) with Time-Series Cross-Section Data," *American Political Science Review* 89 (September 1995).

[68] Complete results are available from the authors upon request.

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TABLE 2
RESULTS OF POOLED REGRESSIONS TESTING FOR THE IMPACT OF INTER- AND INTRAPARTY COMPETITION ON CHARGES OF DC MALFEASANCE, FIRST ELEVEN POSTWAR LEGISLATURES

Independent Variable	*Model 1 (Vote Share)*	*Model 2 (Seat Share)*	*Model 3 (Marginality)*
INTRA	0.0482***	0.0503***	0.0404***
	(0.0229)	(0.0234)	(0.0249)
INTER	0.3501	−0.0325	0.0743
	(0.4185)	(0.0675)	(0.0319)
N	309	309	309
Adjusted r^2	0.5114	0.5100	0.5161
Prob.> *chi*^{2}	0.0000	0.0000	0.0000

*significant at the 0.1 level or better; **significant at the 0.05 level or better; ***significant at the 0.01 level or better; panel-corrected standard errors in parentheses

the coefficients for INTER are not. This result obtains in regressions using all three ways of lagging the variable INTER, although the table reports only one.[69] Finally, note that our two political variables alone account for about half of the variation in the dependent variable in each of the three models, underscoring the importance of political explanations of high-level corruption. Because the units of measurement are not intuitively meaningful, we cannot interpret the size of the effect of intraparty competition on alleged malfeasance.

Our results indicate that charges of malfeasance against DC Deputies for nonopinion crimes tend to be positively affected by intraparty competition and that they are uncorrelated with interparty competition. The absence of significant results for any of the measures of interparty competition that we used corroborates the standard interpretation of the Italian electorate as largely frozen in its electoral orientations.[70] Competition with other parties was generally so limited that it did not systematically and significantly affect the likelihood of DC candidates engaging in illegal fund-raising activities. Instead, it seems likely that competition with other parties was mainly handled with resources distributed by the DC's national party headquarters. Competition with

[69] To facilitate presentation, coefficient estimates for election-year dummy variables are not included; they are significant for every legislature starting in 1972 except 1976.

[70] Arturo Parisi and Gianfranco Pasquino, eds., *Continuità e mutamento elettorale in Italia: le elezioni del* 20 *giugno* 1976 *e il sistema politico italiano* (Bologna: Il Mulino, 1977); in English, see Arturo Parisi and Gianfranco Pasquino, "Changes in Italian Electoral Behavior: The Relationship between Parties and Voters," in Peter Lange and Sidney Tarrow, eds., *Italy in Transition: Conflict and Consensus* (London: Frank Cass, 1980).

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other candidates from within their own party drove the allegedly illegal behavior exhibited by Christian Democratic deputies.

To examine possible temporal variation across legislatures, Table 3 reports the results of separate regressions for each of the eleven legislatures.[71] The results in Table 3 are statistically significant by conventional standards for our measure of intraparty competition for four of the six legislatures starting with the VI, that is, as of the 1972 legislative elections. As of this date DC intraparty competition significantly affected charges of malfeasance against DC deputies. Interparty competition, by contrast, is significant only in 1976—when it had the effect opposite of the one predicted, lowering the number of incidents of suspected malfeasance—and again in the final legislative elections studied, when an extremely large proportion of deputies were subject to investigation (substantially increasing the number of cases included in the analysis). Like the earlier pooled analysis, these results also corroborate the overriding importance of intraparty competition in affecting charges of malfeasance against DC deputies and the relative unimportance of interparty rivalry.

The 1974 change of law regarding the public financing of Italy's political parties makes us suspect this may have contributed to the fact that the results are statistically significant only starting with elections to the VI Legislature. To investigate this, we performed two Chow tests,[72] looking for structural breaks in the data, one where we broke the data as of 1972 and a second where we broke it as of 1976. The resulting F values enable us to reject the null hypothesis that there is no structural break at the 0.001 level for both tests. This corroborates the impression that the data are characterized by an important change as of the early or mid-1970s, but we cannot tell if the break occurred just prior to the 1974 law—which was passed in the middle of the VI Legislature—or just after. Given the coincidence in timing, however, we suspect that political corruption in Italy was reinforced by the 1974 change in the law regulating the public financing of political parties. One interpretation is that the law allowed the judiciary to proceed with investigations of politicians suspected of wrongdoing more aggressively

[71] In the results reported interparty competition is operationalized as the loss of the share of DC seats in the district at time t over t_{-1} (model 2). The result for the first postwar election is therefore omitted, since there was no preceding election. While the other two models yield similar results, they are not presented to facilitate exposition. Complete results are available from the authors upon request.

[72] Briefly stated, the Chow test works as follows. First, we set up a dummy variable, PERIOD, which equals 1 after 1972 (or 1976) and 0 otherwise. Then we create two interaction variables, PXINTER and PXINTRA, by taking the product of PERIOD and INTER and the product of PERIOD and INTRA. Finally, we examine whether the coefficients of PERIOD, PXINTER, and PXINTRA are jointly different from 0.

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TABLE 3

RESULTS OF REGRESSIONS TESTING FOR THE IMPACT OF INTER- AND INTRAPARTY COMPETITION ON CHARGES OF DC MALFEASANCE, FIRST ELEVEN POSTWAR LEGISLATURES[a]

(MODEL 2, SEAT SHARE)

Variable	*Leg. I 1948–53*[b]	*Leg. II 1953–58*	*Leg. III 1958–63*	*Leg. IV 1963–68*	*Leg. V 1968–72*	*Leg. VI 1972–76*	*Leg. VII 1976–79*	*Leg. VIII 1979–1983*	*Leg. IX 1983–87*	*Leg. X 1987–92*	*Leg. XI 1992–94*
INTRA	N/A	−.009	.026	−.012	−.001	.102**	.052	.063*	.146**	.012	.850***
		(.026)	(.031)	(.034)	(.024)	(.047)	(.036)	(.040)	(.065)	(.041)	(.269)
INTER	N/A	.159	.135	−.089	−.052	.228	−.313**	−.233	−.424	.070	.428*
		(.110)	(.125)	(.154)	(.110)	(.250)	(.144)	(.197)	(.355)	(.205)	(.274)
INTERCEPT	N/A	.067	.046	.091*	.062*	.018	.019	.038	.000	.137*	−.198
		(.048)	(.051)	(.055)	(.035)	(.062)	(.046)	(.056)	(.073)	(.072)	(.179)
N	N/A	30	31	31	31	31	31	31	31	31	31
r^2		.035	.043	.016	.007	.174	.205	.141	.246	.004	.270

*significant at the 0.1 level or better; **significant at the 0.05 level or better; ***significant at the 0.01 level or better

[a]Robust standard errors are in parentheses.

[b]N/A = not available because no prior election.

than before, when legal grounds for prosecution were weaker, even in cases of accepting kickbacks. But it is also possible that the law "created" political corruption by the simple fact of criminalizing previously licit behavior.

II. Interpreting the Results: Cultural Diffusion versus Legal Change

Why are charges of malfeasance against Christian Democratic deputies significantly associated with intraparty competition only as of the early 1970s and not earlier? This finding can be interpreted in at least two distinct ways. The first is institutional: the 1974 change in campaign financing laws criminalized behavior that had previously been legal. Systematic changes in behavior among DC politicians did not necessarily occur over the period studied. Supporting this hypothesis is the fact that in the 1990s, when the investigations of a majority of members of the Italian parliament occurred, a vast number of them were charged with crimes pertaining to the violation of the law on party financing. Results presented above suggest that the data display a structural break about 1974.

An alternative interpretation is evolutionary. According to this view, factionalism generated corruption early in the postwar era, but the relationship was weak and unsystematic. Eventually, as increasingly large numbers of Christian Democratic deputies used ever larger sums of illicitly gained campaign funds, the relationship became systematic. By the mid-1970s it was substantial enough to generate a statistically significant coefficient in regression analysis.

Arguably, the latter is the conventional interpretation found in the Italian literature, according to which the illegal use of funds for political campaigns begins on a piecemeal and occasional basis. Over time—assuming that nothing, such as expulsion from public office, interrupts the growth of such a system—selection mechanisms make corruption in campaign financing the norm rather than the exception. Alessandro Pizzorno has argued that political corruption may become endemic when the funds gained provide a sufficient advantage to the corrupt such that they enjoy a higher probability of winning each electoral encounter.[73] In such a situation, politicians who are not prepared to adopt such strategies are wiped out (that is, they lose their seats), while those who are not initially corrupt but manage to survive electorally are

[73] Pizzorno (fn. 15).

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forced to lose whatever scruples they may have held and imitate their more successful colleagues.

Eventually we intend to study this question directly by examining the career paths of DC politicians charged with corruption. Data limitations currently prevent this.[74] For the moment, we examine a specifically cultural variant of the evolutionary argument. The logic of this is well known, thanks to the work of Robert Putnam.[75] As Putnam, like others before him,[76] has argued, southern Italy exhibits distinct patterns of political culture, patterns marked by high rates of preference voting, low turnout in referenda, low newspaper readership, and a scarcity of sports and cultural associations[77]—as well as, presumably, higher rates of organized crime and political corruption.

The cultural perspective can be easily adapted to the phenomenon of political corruption. The standard story is that corruption spread in Italy from south to north along with the clientelistic political machine of the DC. Hine, for instance, argues this quite explicitly, as well as offering a plausible explanation for the initial emergence of corruption in the Italian south:

> Any region starved of resources, but operating in a highly centralised state, with a uniform tax and welfare system, will demand of its representatives that they spend a major part of their time competing for resources for their localities. . . . If the region in question has . . . a high level of organised crime with perpetrators anxious to use their resources to infiltrate the political and financial systems to recycle illegal earnings and buy political protection . . . that too will ultimately have an effect on national political life.[78]

And he sums up the consequences by claiming that the political system in the north "seems to have acquired many of the characteristics originally most prevalent in the south."[79]

While the same study later examines institutional factors responsible for the widespread political corruption observed throughout Italy, including the preference vote, the geographic contagion argument is widespread in the literature. It holds that corruption is not endogenous to institutions of the Italian political system but rather was introduced

[74] Because of difficulties in matching the records by name, we have not been able to combine our data sets listing the legislators charged with malfeasance with the data sets we compiled reporting the number of preference votes DC candidates received. We expect to do this in the future.

[75] Robert Putnam, *Making Democracy Work: Civic Traditions in Modern Italy* (Princeton: Princeton University Press, 1993).

[76] Most notably, Edward C. Banfield, *The Moral Basis of a Backward Society* (Glencoe, Ill.: Free Press,1958).

[77] Putnam (fn. 75), 96.

[78] Hine (fn. 16), 145.

[79] Ibid.

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from outside of it, thanks to historical and cultural traits of southern Italy, including those involving the Mafia and organized crime.[80] These factors interacted with the long-standing economic gap between northern and southern Italy to drag southern politicians into corrupt activities, which then spread to the rest of the country via a process of cultural contagion.

To examine this interpretation, we map intraparty competitiveness and suspected corruption over the course of the postwar era. Figures 2–5 show both intraparty DC competition and charges of corruption against Christian Democratic MPs by electoral district for the two earliest postwar legislatures and the two final ones under the old electoral system.[81] The values of each variable were pooled across Italy's first eleven postwar legislatures so that the shading on the maps is comparable across legislatures. The pooled data were quartiled, and each quartile was shaded differently, representing least to most intraparty competitiveness/corruption. The only exception to this standardization procedure was the measure of factionalism for the 1992 election, which allowed only a single preference vote to electors. As pooling these results with those from other elections would artificially reduce the variation across electoral districts in 1992, we did not do so.

Examination of the maps showing intraparty competition confirms the conventional wisdom that the use of preference votes has always been more extensive in southern Italy than in northern Italy.[82] Intraparty competitiveness appears relatively limited in the first postwar election. The maps corroborate other studies that have argued that intraparty competition and factional conflict in the DC underwent an initial increase in the early to mid-1950s.[83] There is no substantial evidence of a nationalization of intraparty competitiveness in the DC even in the late 1980s or early 1990s. Preference voting remains a largely southern activity.

The maps showing the proportion of DC members of parliament charged with nonopinion types of malfeasance in the early and later postwar legislatures are quite different from what the cultural hypothesis predicts. First, politicians charged with wrongdoing were not con-

[80] This view turns on classing the Mafia as a cultural phenomenon, not an institution.

[81] The intervening maps are not presented for reasons of space but are available from the authors upon request.

[82] See Franco Cazzola, "Partiti, correnti e voto di preferenza," in Mario Caciagli and Alberto Spreafico, eds., *Un sistema politico alla prova: studi sulle elezioni politiche italiane del* 1972 (Bologna: Il Mulino, 1975), 143; and Giovanni Schepis, "Analisi statistica dei risultati," in Alberto Spreafico and Joseph La Palombara, eds., *Elezioni e comportamento politico in Italia* (Milan: Edizioni di Comunità, 1963), 376.

[83] Zuckerman (fn. 50), 110–11.

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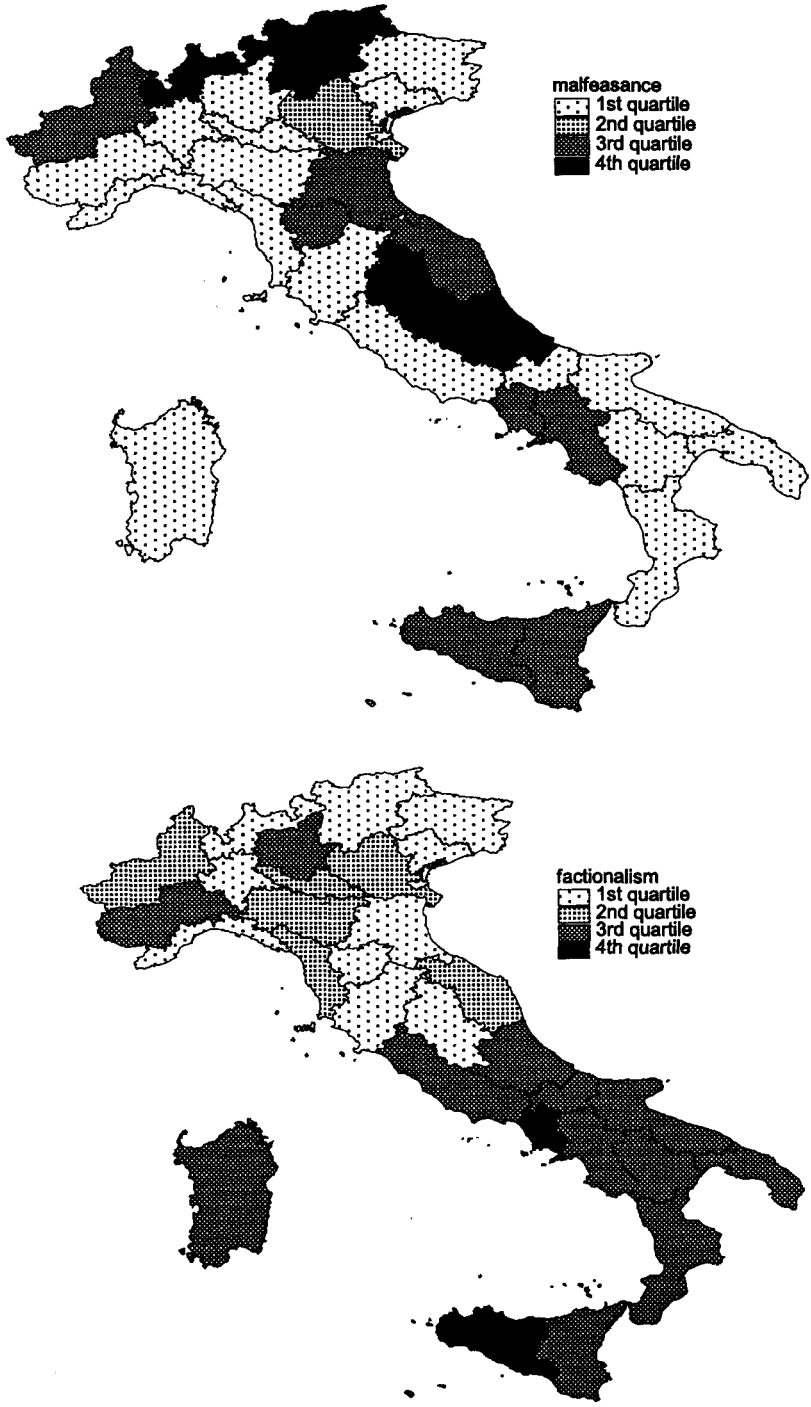

FIGURE 2
DC INTRAPARTY COMPETITIVENESS AND CHARGES OF MALFEASANCE AGAINST DC MP'S: I LEGISLATURE, 1948–53

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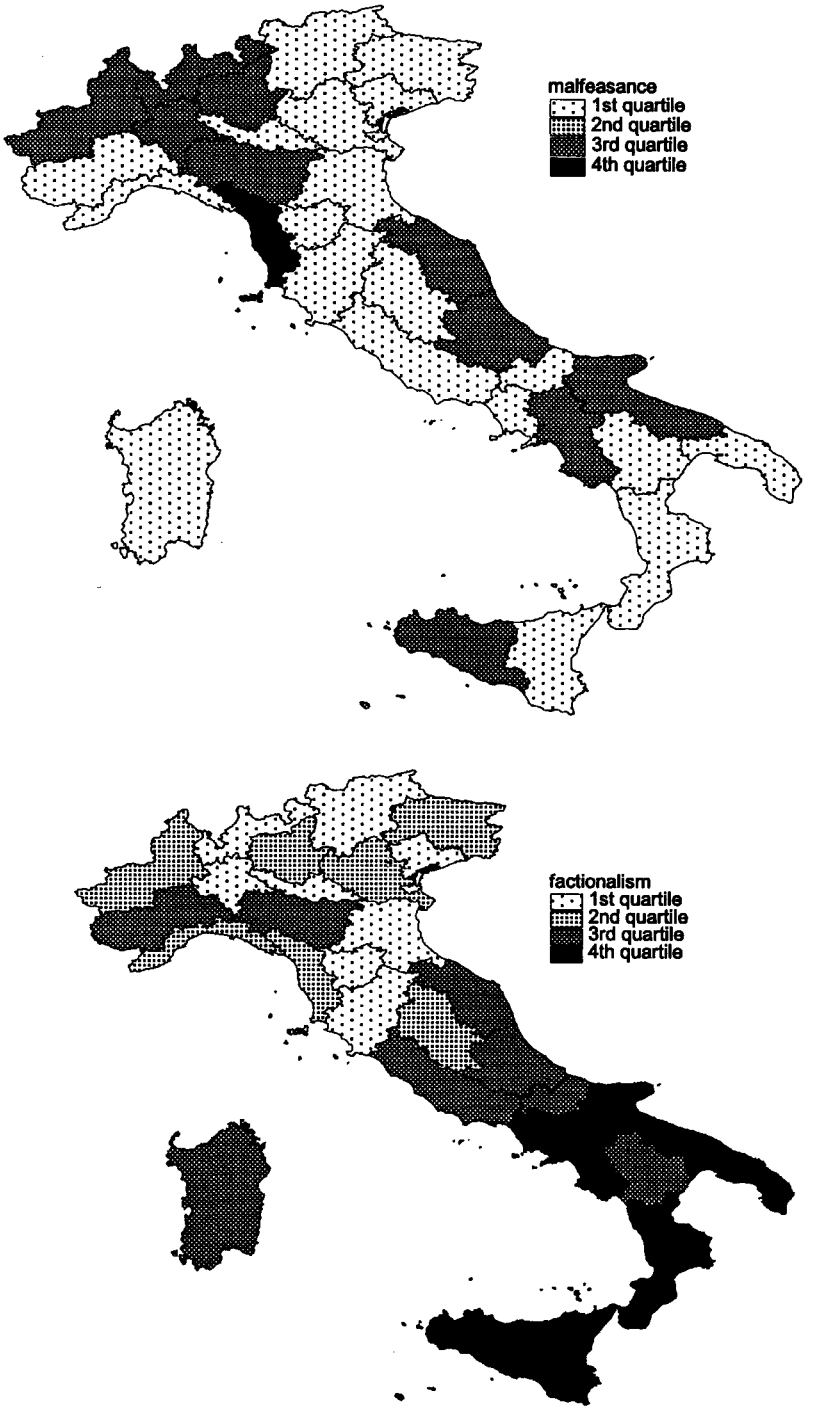

FIGURE 3
DC INTRAPARTY COMPETITIVENESS AND CHARGES OF MALFEASANCE AGAINST DC MP'S:
II LEGISLATURE, 1953–58

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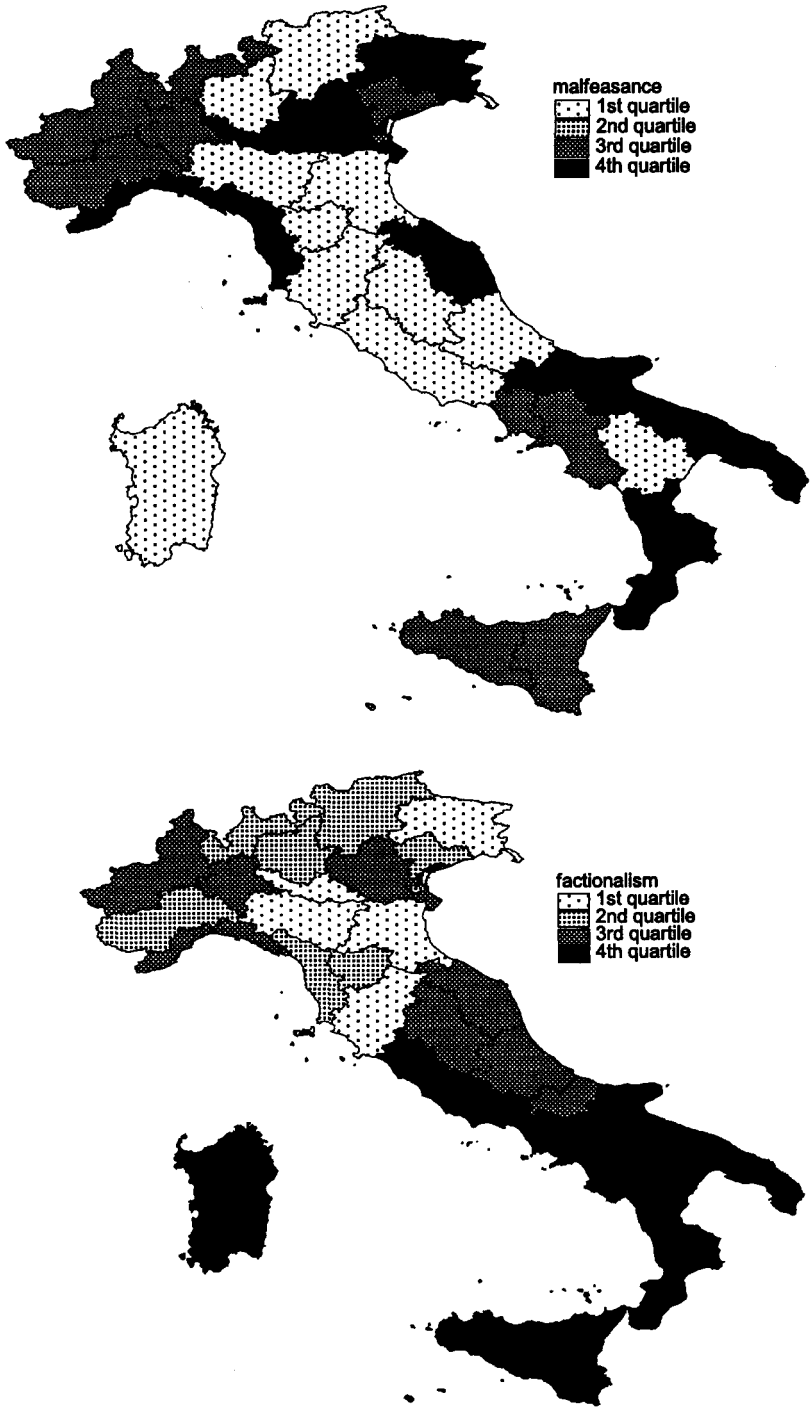

FIGURE 4
DC INTRAPARTY COMPETITIVENESS AND CHARGES OF MALFEASANCE AGAINST DC MP'S: X LEGISLATURE, 1987–92

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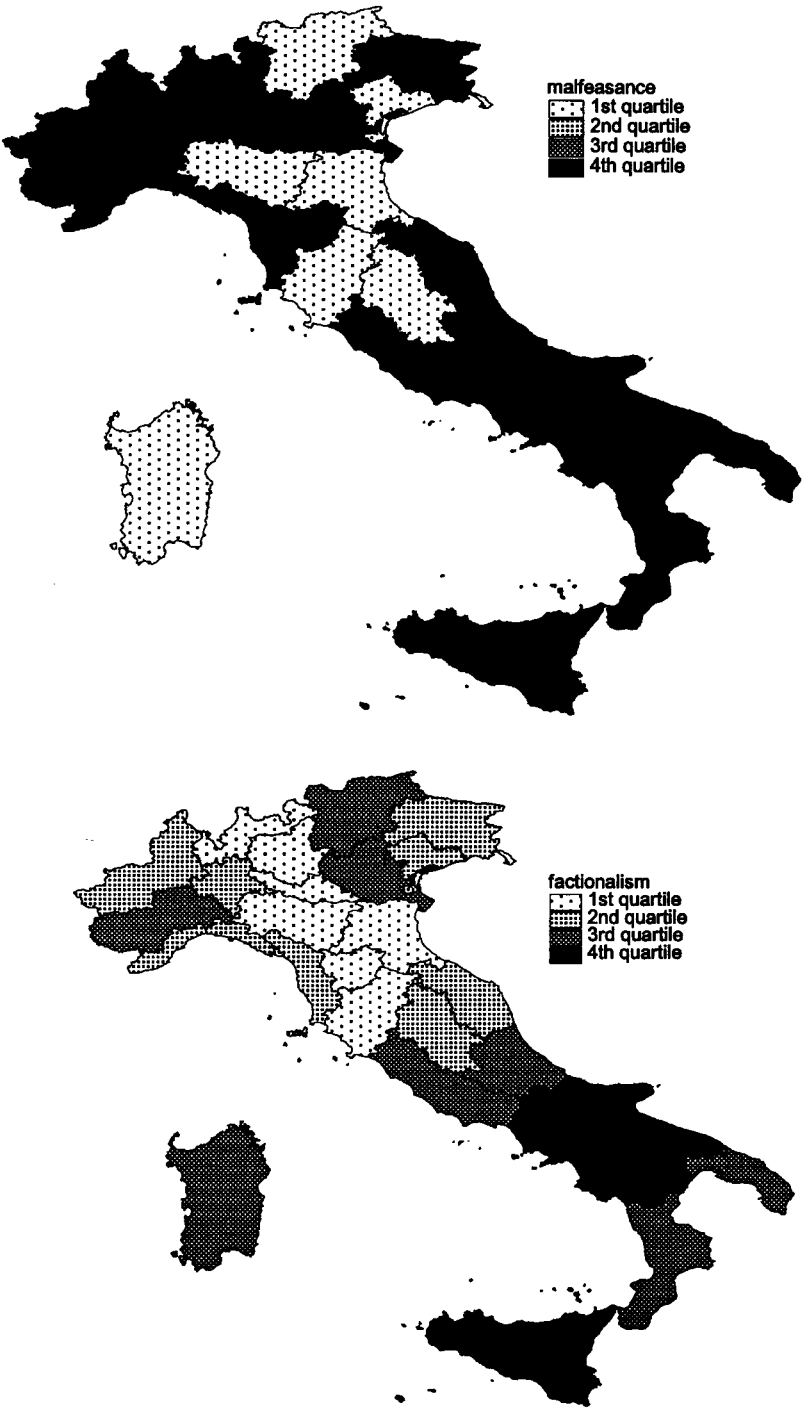

FIGURE 5
DC INTRAPARTY COMPETITIVENESS AND CHARGES OF MALFEASANCE AGAINST DC MP'S:
XI LEGISLATURE, 1992–94

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centrated in the south in the early postwar years. The first two legislatures saw only a single southern district in the highest quartile of those charged.[84]

Second, as charges of wrongdoing became more frequent later in the postwar era, they were also distributed across all parts of Italy. (In the VIII Legislature, for instance, nine of Italy's thirty-two electoral districts appear in the highest quartile of proportion of MPs charged, double the number of districts so classed in the I Legislature.) Suspected wrongdoing appears at high levels in districts from the top of the peninsula to the bottom of the boot. By the time the Clean Hands investigations were under way, political corruption in Italy had become a national phenomenon. In the final legislature we study, twenty-three districts appear in the highest quartile, and eleven of these are located in the south.

It is possible, of course, that the measure employed here has produced a geographically misleading picture of suspected criminal behavior among Christian Democratic politicians. The southern judiciary may have been less prone than the northern judicial offices to investigate suspected wrongdoing by DC deputies, perhaps because organized crime often figured as an intermediary in political corruption in the south. We have no way of verifying whether data on the RAP are biased in this way, although it seems probable. But if they are, then the relatively high incidence of suspected wrongdoing among northern DC deputies who served in the first and second postwar legislatures makes us suspect that were more valid data available, they would only *reinforce* the finding that suspected political corruption has always been dispersed across the Italian electoral districts and was not initially concentrated in the south. The reason is that if the present data set is biased, this bias means that we are undercounting suspected corruption in the south in the early legislatures. A less biased data set would therefore show suspected corruption in the earlier postwar era not only in the north but also in the south (unless the underreporting of wrongdoing in the south is so massive in the early period as to dwarf even the relatively high levels of charges brought against deputies in some northern districts).

The requests to remove parliamentary immunity of DC deputies suggest that political corruption, rather than emerging out of Italy's supposedly familial southern political culture as Banfield and Putnam would contend, was instead a product of its postwar political institu-

[84] In the III Legislature four southern districts were classed in the highest quartile of suspected malfeasance.

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tions. It was manifest at high levels in various electoral districts across Italy from relatively shortly after World War II.

III. Conclusions

We have demonstrated that Italy's use of the preference vote appears to have contributed significantly to the development of extensive, high-level political corruption in the postwar era. This relationship is apparent as of the early 1970s, perhaps in connection with the change in a law regulating party financing. We interpret these findings as demonstrating that political corruption emerged as a result of the search for campaign funds by DC members of parliament in their competition with candidates from the same party. We find no evidence in our data that corruption was initially significantly higher in southern Italy or that it spread from south to north.

Our finding that interparty competition typically made no statistically significant contribution to political corruption should not be interpreted as meaning that characteristics of the Italian party system were not relevant to the growth of corruption there. Underlying the growth of corruption among DC deputies was the justifiable belief that the PCI did not constitute a genuine threat to the DC's hold on national office[85]—that the combination of political veto by the United States to the entry of a Communist Party into a NATO government and anti-communism on the part of much of the Italian electorate would continue to allow the DC to remain permanently in office. Indirect corroboration of this interpretation comes from the fact that only after the fall of the Berlin Wall, the subsequent demise of the U.S. veto on PCI participation in government, and the related shifts in electoral behavior was the system of political corruption exposed and then brought down.[86] Until then, the DC and its allies could continue to believe that engaging in extensive illegal activities would carry no major political consequences. This "arrogance of power," as it is commonly called in Italy, pushed Italy well off the regression line in the comparative extent of corruption.

[85] Sidney Tarrow, "Maintaining Hegemony in Italy: 'The Softer They Rise, the Slower They Fall!'" in T. J. Pempel, ed., *Uncommon Democracies: The One-Party Dominant Regimes* (Ithaca, N.Y.: Cornell University Press, 1990).

[86] We do not believe, however, that the end of communism was a significant cause in exposing Italian political corruption or in the collapse of the postwar party system; for a discussion, see Miriam A. Golden, "International Sources of the Collapse of Rent-Seeking Regimes: Hypotheses Drawn from the Italian Case" (Paper presented at the Joint Sessions of the European Consortium for Political Research, Grenoble, France, April 6–11, 2001).

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To return to our opening puzzles, because of the absence of political alternation, competition of elites in Italy was abnormally restricted. This prevented a proper functioning of legal and constitutional protections against political corruption, magnifying the pernicious effects of specific aspects of the electoral system. These institutional aberrations permitted excessive and persistent misgovernment by elected officials. The collapse in the 1990s of the international and domestic conditions that bred such a poorly functioning democratic political order bode well for a substantial, and possibly permanent, reduction of political corruption in Italy.[87] Our analysis demonstrates the importance of political institutions in significantly affecting the degree of political corruption and, by extension, the extent to which deliberate and appropriate institutional modifications may reduce the incentives for corrupt behavior by legislators.

[87] Given this, readers may wonder why the TI index for Italy did not show a substantial decline even at the turn of the millennium. The reason is that the TI index fails to distinguish bureaucratic from political corruption. In fact, since it is based on surveys of businesspeople, it may even reflect mainly bureaucratic corruption. We believe that bureaucratic corruption has remained relatively high in Italy (in part due to the absence of thoroughgoing reform of the public administration), whereas political corruption has fallen drastically since the change of regime in the mid-1990s. On the persistence of bureaucratic corruption, see Alberto Vannucci, "Inefficienza amministrative e corruzione," *Rivista Trimestrale di Scienza della Amministrazione* 44, no. 1 (1997).

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Research Note

IMPROVING FORECASTS OF STATE FAILURE

By GARY KING and LANGCHE ZENG*

I. INTRODUCTION

"STATE failure" refers to the complete or partial collapse of state authority, such as occurred in Somalia and Bosnia. Failed states have governments with little political authority or ability to impose the rule of law. They are usually associated with widespread crime, violent conflict, or severe humanitarian crises, and they may threaten the stability of neighboring countries. States that sponsor international terrorism or allow it to be organized from within their borders are all failed states. Since the consequences for the citizens of these states can be very severe and the costs to the international community of rebuilding the states are often substantial, there has long been considerable interest in developing methods of risk assessment and early warning systems in the hope that foreign aid could be directed to prevent states from failing. In 1994, with these goals in mind, the U.S. government, at the behest of Vice President Gore, established and funded the State Failure Task Force, a panel of distinguished academic social scientists, experts in data collection, and consultants in statistical methods. Although the

* The authors have no formal or informal relationship with the State Failure Task Force or the task force's sponsor, the U.S. Central Intelligence Agency. We thank Matt Baum for research assistance; Jim Alt, Aslaug Asgeirsdottir, Bob Bates, Ben Bishin, Lee Epstein, Jim Fearon, Charles Franklin, Jeff Frieden, Kristian Gleditsch, Jack Goldstone, David Laitin, Chris Murray, Kevin Quinn, Ken Scheve, Alan Stain, Ben Valentino, Jonathan Wand, and Mark Woodward for helpful discussions; the State Failure Task Force for collective written comments; Bob Bates for his suggestion that we take on this project; and the National Science Foundation (SBR-9729884, SBR-9753126, and IIS-9874747), the National Institutes of Aging, and the World Health Organization for research support. For providing us copy of the task force data, we thank task force members Bob Bates and Monte Marshall. All data referenced in this article are available at http://gking.harvard.edu, and for making this pos sible we are thankful for the guidance and assistance of Dick Cooper and the efforts of attorneys Kim Budd, Bob Donin, Allan Ryan, and Bob Iulioano in the Harvard University General Council's office.

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task force does not use classified information, the data amassed are nonetheless impressive: more than a thousand variables, each carefully collected and documented and many with value added beyond what is available from other sources. (See Appendix 1.) The task force, still in operation, has produced over two hundred pages of widely distributed formal reports and analyses[1] and several published article-length summaries.[2] This work has received attention in the popular news media[3] and "has gained substantial visibility and credibility among those responsible for the analysis of global security and for planning U.S. foreign policy,"[4] an uncommon achievement for quantitative analyses in this field.

The task force reports were aimed at policymakers, but the research has been of considerable interest to the scholarly community as well. The authors make stunning claims about their success at forecasting these highly heterogeneous and idiosyncratic events, and they draw numerous important inferences about the causes of a critical and understudied political phenomenon. In this article we provide the first independent scholarly evaluation of the methods, analyses, and claims of the State Failure Task Force. We first identify and correct several methodological errors and then show how to use the task force's data to improve forecasts of state failure substantially beyond even appropriately corrected versions of its statistical models. We hope that this article can then help to connect the goals and efforts of the policy and academic communities in understanding and perhaps even addressing this critical global problem. The work analyzed here also touches on an unusually wide range of underutilized methods and relevant methodological issues; we seek to clarify some of these so that scholars can use them more productively.

[1] Daniel C. Esty, Jack Goldstone, Ted Robert Gurr, Pamela T. Surko, and Alan N. Unger, *Working Papers: State Failure Task Force Report* (McLean, Va.: Science Applications International Corporation, 1995); Daniel C. Esty, Jack Goldstone, Ted Robert Gurr, Barbara Harff, Pamela T. Surko, Alan N. Unger, and Robert S. Chen, *The State Failure Task Force Report: Phase II Findings* (McLean, Va.: Science Applications International Corporation, 1998).

[2] Daniel C. Esty, Jack Goldstone, Ted Robert Gurr, Barbara Harff, Pamela T. Surko, Alan N. Unger, and Robert S. Chen, "The State Failure Project: Early Warning Research for U .S. Foreign Policy Planning," in John L. Davies and Ted Robert Gurr, eds., *Preventive Measures: Building Risk Assessment and Crisis Early Warning Systems* (Lanham, Md.: Rowman and Littlefield, 1998); Daniel C. Esty, Jack Goldstone, Ted Robert Gurr, Barbara Harff, Marc Levy, Geoffrey D. Dabelko, Pamela T. Surko, and Alan N. Unger, "The State Failure Report: Phase II Findings," *Environmental Change and Security Project Report* 5 (Summer 1999).

[3] For example, Tim Zimmermann, "CIA Study: Why Do Countries Fall Apart? Al Gore Wanted to Know," *U.S. News and World Report,* March 12, 1996.

[4] Esty et al. (fn. 2, 1998), 27–38; and, e.g., John C. Gannon, *The Global Infectious Disease Threat and Its Implications for the United States* (U.S. National Intelligence Council, http://www.cia.gov/cia/publications/nie/report/nie99-17d.html, 2000).

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II. Task Force Data and Models

According to the task force, a "state failure" consists of revolutionary wars ("sustained military conflicts between insurgents and central governments, aimed at displacing the regime"), genocides and politicides ("sustained policies by states or their agents and, in civil wars, by contending authorities that result in the deaths of a substantial portion of members of communal or political groups"), and adverse or disruptive regime transitions ("major, abrupt shifts in patterns of governance, including state collapse, periods of severe regime instability, and shifts toward authoritarian rule)."[5] The authors intentionally included fairly diverse events in their definition of state failure in order to follow the guidelines of policymakers as articulated to the task force. This may be a reasonable starting point, in part, because it increases the number of events in the data set, but also because it assumes that the benefits of having more events outweigh the costs of lower predictive ability and model incoherence resulting from increased heterogeneity in the outcome variable. In this article we use the dependent variable as conceptualized and measured by the task force (in order to isolate the effects of our methodological corrections), but in all likelihood a different causal structure underlies each component of the constructed concept of "state failure." Although our very flexible model will pick up some of these differences, we recommend that future researchers experiment with fitting different models to each component. We return to this issue in the conclusion.

According to the definition used by the task force, 127 state failures had commenced between 1955 and 1998 in some of 195 distinct countries in the data set.[6] Thus, the outcome variable is state failure, which we denote as Y_i, coded 1 for country-years in which a state failure started and 0 for country-years with no failure. Since the goal of the task force was to explain the onset of state failure (incidence rather than prevalence, in epidemiological terms), subsequent years in which a country remained in a state of failure are dropped.

The task force collected its data via a case-control design, which is especially efficient for rare events data.[7] First, it collects all cases of fail-

[5] Esty et al. (fn. 2, 1998), 27–38.

[6] Fewer than 195 countries appear in the data set in any one year. For example, Germany, East Germany, and West Germany are three separate items in this count, even though for any one year in the data set, either Germany or East and West Germany appear. Countries enter the data set in 1955 or when they first came into existence if later; countries remain in the data set after an episode of failure. In addition, the task force was required by the U.S. government to omit the United States from all analyses. They also omitted countries with fewer than half a million people.

[7] Norman E. Breslow, "Statistics in Epidemiology: The Case-Control Study," *Journal of the American*

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ure. Then for each failure it randomly draws three nonfailures from the same year. The advantage of this scheme is clear in comparison with a random sample, which could by chance miss many important events. The 1:3 ratio is not required for case-control studies, although all cases of failure were used and every additional control adds progressively less information after the first, so there is little point of continuing much further if data collection is expensive. The task force then coded hundreds of explanatory variables.[8]

To choose a model, the task force authors lagged all the explanatory variables by two years so that their models would predict two years into the future. They then conducted an extensive search process fitting their entire data set via logistic regression numerous times to different specifications (that is, sets of explanatory variables). They used genetic algorithms, stepwise logistic regression, and other informal procedures to examine other specifications. Listwise deletion was applied to each specification by deleting a country-year if any variable in the model was missing (so that each logit model was run on a different set of country-years). They also report performing a first cut, narrowing the list of variables to thirty-one on the basis of univariate t or chi-square tests. Then "combinations of two, three, five, and up to 14 variables contained in the 31-variable set were examined together in an inductive approach to specifying the most accurate analysis or model."[9] Even this second step was constrained by the qualitative knowledge of the authors, as well as by some external rules, since the number of combinations of explanatory variables that could have been tested in this way is over 773 million (if each combination took 10 seconds to run and evaluate, it would take 245 years to complete them all). The summary measure they used to judge the quality of each model was not identified.

This process led the task force to choose a simple logistic regression model with three variables: *democracy* (the standard Polity III democracy and autocracy scores, collapsed into the categories of full democracy, partial democracy, and autocracy and coded as two dummy variables), *trade openness* (the log of imports plus exports as a percentage of GDP), and *infant mortality* (the log of the ratio of the infant mortality rate to the world median). (This entire process was repeated using

Statistical Association 91 (March 1996); Gary King and Langche Zeng, "Explaining Rare Events in International Relations," *International Organization* (forthcoming), preprint at http://gking.harvard.edu.

[8] We focus only on the task force's so-called "global model." Its data set includes 1,231 variables, although many of these are recodes of other variables or markers of problems with individual observations. Although the task force writings indicate that it used only the case-control data, its data set contains at least some information and always Y_i for every country.

[9] Esty et al. (fn. 2, 1998).

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"neural network clustering," the specific version of which the authors do not identify, although they conclude that it does no better than their logit model.)

There is much to criticize in this relatively atheoretical search procedure, but without a theory that rules out hundreds of the variables in the task force data set, uniformly superior statistical procedures do not yet exist (although see West et al.)[10] The task force's ultimate model choice was parsimonious and (aside from the case-control corrections, which had enormous effects) was not easy to surpass.

Since the analyses conducted by the task force and used by policymakers are based on continuously updated data, variables, and methods, we requested and received the data and results from their current model of choice, which still includes the same three variables. In all other ways, too, differences between the data used in their published report and those used in the newer version were minor and always inconsequential for the points discussed herein. Except where noted, we base all our analyses and comparisons on these new data. In the final task force model, 108 failures and 315 nonfailures were included, and 85 other observations were dropped due to the application of listwise deletion.

III. Correcting Methodological Errors

In this section we discuss methodological errors and opportunities for improving on the State Failure Task Force statistical analyses. We consider problems stemming from (1) their case-control design, (2) the way they evaluate forecasting performance, (3) the way they distinguish in-sample fit from out-of-sample forecasts, and (4) the way they treat missing data.

Case-Control Problems

The task force collects data by selecting on the dependent variable, a procedure known to cause bias.[11] As the task force makes no correction for this problem, all its estimates are therefore biased, in most cases quite severely.

Most obviously, the marginal distribution of Y is biased in case-control sampling. In the present case, the fraction of state failures in the

[10] Mike West, Joseph R. Nevins, Jeffrey R. Marks, Rainer Spang, and Harry Zuzuan, "Bayesian Regression Analysis in the 'Large p, Small n' Paradigm with Application in DNA Microarray Studies" (Manuscript, Duke University, 2000).

[11] For example, Gary King, Robert O. Keohane, and Sidney Verba, *Designing Social Inquiry: Scientific Inference in Qualitative Research* (Princeton: Princeton University Press, 1994).

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TABLE 1
STATE FAILURE TASK FORCE LOGISTIC REGRESSION RESULTS

	Coefficient	*Standard Error*
Log (infant mortality)	.62	(.20)
Log (trade openness)	−.85	(.22)
Full democracy	−.16	(.40)
Partial democracy	2.00	(.33)
Constant	1.88	(.84)
Prior-corrected constant	−1.12	(.84)

data, $\bar{y}$, is $\bar{y} = 0.255$, whereas the fraction of failures in the universe of country-years, which we denote τ, is only $\tau = 0.0168$.

Although the costs of taking no action are large, case-control designs (unlike most methods of selecting on Y_i) are exceptionally easy to correct even without additional statistical assumptions or complicated models.[12] Intuitively, we must correct for the way the sample fraction of failures y misrepresents the population fraction, and so the technical fix in any particular statistical model will always involve weighting the sample to move from $\bar{y}$ to τ. Researchers call this procedure *prior correction*. For example, in logistic regression (and any other multiplicative intercept model, such as the neural network committee model we introduce below) prior correction involves merely subtracting $\ln[(1-\tau)/\tau][\bar{y}/(1-\bar{y})]$ from the estimated constant term. Although the slope coefficients require no correction, the nonlinearities of the logit model mean that forecasts and estimates of causal effects (and almost all other quantities of interest) cannot be computed without the constant term.[13] We now demonstrate the bias in the task force's forecasts and causal estimates.

We begin with a replication of the task force's best current model, which appears in Table 1. Substituting into the equation in the previous

[12] King and Zeng (fn. 7).

[13] Let X be a vector of k explanatory variables, including a constant term, and X_0 and X_1 each denote $1 \times k$ vectors of values of the explanatory variables (e.g., with one variable changing and the others remaining constant at their medians between X_0 and X_1). Quantities of interest usually include raw probabilities of failure, relative risks, and first differences. The first difference, $\Pr(Y = 1|X_1) - \Pr(Y = 1|X_0)$, is the increase in probability, and the relative risk, $\Pr(Y = 1|X_1)/\Pr(Y = 1|X_0)$, is the factor by which the probability increases, when the explanatory variables change from X_0 to X_1.

In one special case, the relative risk can be approximated indirectly without the constant term via an odds ratio, which in logit is a function of the slopes only. However, this approximation is accurate only as $\tau \to 0$, which is the assumption that no state is ever at risk of failure, in which case there would not be much point in forecasting state failure in the first place (although the bias can be small if τ is very small). In addition, the assumption implies implausibly that $\Pr(Y = 1|X) = 0$ for any X and that all first differences are 0. For details, see Gary King and Langche Zeng, "Estimating Risk and Rate Levels, Ratios, and Differences in Case-Control Data," *Statistics in Medicine* (forthcoming), preprint at http://gking.harvard.edu.

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TABLE 2
SELECTED PREDICTED PROBABILITIES[a]

State	*Year*	*Task Force*	*Prior Corrected*
Somalia	1988	.45	.04
Comoros	1995	.66	.09
Chile	1973	.56	.06
Ghana	1972	.55	.06
Ecuador	1970	.58	.06
Brazil	1964	.72	.11
Benin	1963	.72	.11

[a]From the task force logistic regression and from the authors' prior-corrected version.

paragraph, we calculate that the correction factor in this case is 3.0. We thus subtract 3.0 from the logit constant to produce the prior-corrected constant in the last line. (This may seem like a small number, but as we shall see, it has a large effect on the quantities of interest.)

BIAS IN TASK FORCE FORECASTS

Although the task force repeatedly refers to its forecast numbers as "probabilities," they are not probabilities since they were not prior corrected. For an example of the bias in the numbers produced by the task force, consider the simple case with no explanatory variables (or none with an effect). In this situation a predicted probability of failure, based on the global population of country-years or the corrected case-control sample, would equal 0.0168. However, the uncorrected case-control sample yields an estimate fifteen times larger, an incredible prediction that slightly more than a quarter of the states in the world will fail in any one year. When the task force includes explanatory variables, some of the probabilities extend to 0.89, which is implausibly large for this problem.

Table 2 gives a few examples of the overestimates in the task force's predicted probabilities.[14] The first column of numbers in our table, taken from Esty et al.,[15] is labeled "model score" and is explained as the "the predicted probability according to the model."[16] The second column contains the correct predicted probabilities that we computed.[17]

[14] All country-year predictions were highly biased. For this illustration, we chose a few cases that might be familiar and a few that were less familiar. Readers can easily compute the bias in all other country-years using our methods, a hand calculator, and their tables.

[15] Esty et al. (fn. 1, 1998), Table A-7.

[16] Ibid., 57.

[17] We computed these via prior correction from numbers given in Esty et al. (fn. 1, 1998), by using equation 26 in Gary King and Langche Zeng, "Logistic Regression in Rare Events Data," *Political*

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TABLE 3
BIASED AND CORRECTED QUANTITIES OF INTEREST[a]
(WITH 95% CONFIDENCE INTERVALS IN PARENTHESES)

	Relative Risk			*First Difference*		
Autocracy to partial democracy						
task force	3.66	(2.45,	5.61)	.42	(.27,	.55)
corrected	7.04	(3.57,	13.21)	.06	(.03,	.09)
Full to partial democracy						
task force	4.14	(2.37,	7.66)	.43	(.27,	.58)
corrected	8.08	(3.58,	18.59)	.06	(.03,	.10)

[a]The relative risk is the ratio and the first difference is the difference in the probability of state failure when changing the democracy variables, holding other variables constant at their global medians.

By any substantive or statistical measure, the task force estimates are far from accurate and range from 6.54 to 11.25 times too large. The task force authors included several long tables in their reports with numerous estimated probabilities, but unfortunately the lack of prior correction means that every such estimate is incorrect, sometimes by more and sometimes by less than the examples in Table 2. Fortunately, these are easy to correct.

BIAS IN TASK FORCE CAUSAL INFERENCES

We demonstrate the bias in the causal effects estimated from the uncorrected case-control analysis with one key example, highly touted by the task force in all its writings: the effect of democracy on the probability of state failure. Table 3 gives relative risks and first differences (with 95 percent confidence intervals) computed from the original uncorrected model and with appropriate corrections. For example, when a nation moves from autocratic to partial democracy and other variables are held constant at their global medians, the task force's biased estimate is that the probability of state failure more than triples (increases by 3.66). However, the correct estimate (7.04) is nearly twice as large. A similar bias, of about a factor of two (4.14 to 8.08), occurs when moving from full to partial democracy.

Unfortunately, the bias correction does not always increase the size of estimated effects as it happens to with these selected relative risks.

Analysis (forthcoming), preprint at http://gking.harvard.edu. Since the raw data are not needed for this calculation, we stuck to the published version, which was based on a data set that differed slightly from the updated one we used. We also reproduce this with the new data, and there were only very minor differences.

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For example, we also computed first differences for these same causal counterfactuals and found the bias to be in the opposite direction. Table 3 shows that a change from autocracy to partial democracy is estimated with the task force's methods to increase the probability of state failure by 0.42, which is an immense effect. The correction brings this down to a modest 0.06. A similar sevenfold change occurs when correcting the first difference for a change from full to partial democracy. (The direction of bias between the relative risk and first difference results may seem contradictory, but (a/b) and $(a - b)$ are not constrained mathematically to change in the same direction as the estimates, a and b, change; the directions of the bias may also change in other examples.)

This same problem also occurs in the simpler context of comparing the raw numbers the task report reports as probabilities, since the correct probabilities are not preserved in the levels or even in the ratios of or differences between their numbers. For example, when the explanatory variables change from the profile of Somalia in 1988 to that of Brazil in 1964 (both taken from the task force and Table A-7 reproduced in our Table 2), the relative risk increases by a factor of 0.72/0.45 = 1.6 according to their numbers, but a much larger 0.11/0.04 = 2.75 when appropriately corrected. Similarly, the first difference indicates an increase in the probability of 0.72 − 0.45 = 0.27 according to the task force but of only 0.11 − 0.04 = 0.07 when appropriately corrected.[18]

Task force estimates without prior correction do preserve the ranking of the (in-sample) probabilities, but this ranking by itself is not useful for any policy purpose: it indicates only which country has a higher risk of failure, not whether any country is at high enough risk to warrant spending money or risking troops. And, as we will see in the next section, the rankings are not preserved in real out-of-sample forecast probabilities.

These results show that the causal estimates in the task force reports are unreliable and the biases are in otherwise unexpected directions and magnitudes. The biases in other comparisons of relative risks and first differences that we calculated (not shown) vary widely. Fortunately, the

[18] In our discussions with the task force, we learned that they sometimes estimated relative risks in Table 3 indirectly and approximately via an odds ratio (where prior correction is unnecessary; see fn. 13), rather than directly and without prior correction, as assumed here. The indirect approach is also biased except when the expected population of failures becomes 0. The indirect approximation (and even the phrase "odds ratio") is never mentioned in the task force reports or other publications, but if the task force had used it for its written work, then its relative risk estimates computed from the logistic regression in Table 1 are more accurate than indicated in our Table 3. However, the task force estimates of relative risks, such as those computed from the probabilities in Table 2 and described in the text above, would be as biased, and their estimates of probabilities and first differences would be considerably less accurate than we indicate.

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corrections we provide can easily be used in future work to generate accurate figures.

All quantities labeled "causal effects" in this section are calculated based on the assumptions that the counterfactuals necessary for making causal inferences are correct and that the task force's model is appropriate. For the first, the task force assumes that infant mortality and trade openness are causally prior to the level of democracy. This means that if we could exogenously change the level of democracy in a country, then infant mortality and trade openness would not change as a result, as assumed in Table 3. For the second, the task force assumes that if any causes of state failure exist that are causally prior to and uncorrelated with democracy, then they are included in the equation; if an explanatory variable exists that meets these conditions other than democracy, trade openness, and infant mortality, the task force model has additional biases. The first assumption is implausible and unfortunately very hard to correct. The second assumption is by definition unverifiable but is considerably more plausible given the task force's extensive search for other predictive variables. We continue this discussion in Section VI.

Evaluating Forecasting Success

When appropriately corrected, the logistic regression models used by the task force give estimates of the probability of state failure conditional on chosen values of the explanatory variables $\hat{\pi} \Pr(Y = 1|X)$. A separate step, governed by decision theory, is required to decide on the basis of $\hat{\pi}$ whether the state in question will fail or not.

Let C denote the cost of mispredicting a state failure as a nonfailure relative to the cost of mispredicting a nonfailure as a failure. Decision theory tells us that whatever C is, the optimal prediction (in the sense of minimizing total expected cost) is $Y = 1$ when $\hat{\pi} > 1/(1 + C)$ and $Y = 0$ otherwise.[19] Hence, if the two possible mispredictions are equally costly, then $C = 1$ and we would predict that a state will fail when $\hat{\pi} > 0.5$. However, if the cost of mispredicting a state failure is, say, twice as costly as mispredicting a nonfailure, then $C = 2$, and an optimal decision process would predict state failure whenever $\hat{\pi} > 1/3$.

Only by applying decision theory in this way can we compare model outputs to the data and judge our success in prediction. The key, however, is that the value of C must be decided *independently* of the data and statistical results. The task force violated this rule and instead "di-

[19] For example, B. D. Ripley, *Pattern Recognition and Neural Networks* (New York: Cambridge University Press, 1996).

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vided errors evenly between 'false positives' and 'false negatives.'" Of course, the only way to "divide errors evenly" is to inspect the actual values of Y and change C on that basis, which is inappropriate. This is easy to see when forecasting out of sample for genuine policy purposes, since we would not know the future values of Y when C was being chosen.

On the basis of its post hoc adjustment of C, the task force concludes: "The models classify correctly about 70% of historical cases. A model with 70% accuracy two years in advance would correctly identify about two out of three failures and two out of three stable countries." (Using the task force's methods with its new data results in nearly the same number.) Since C cannot be adjusted post hoc in real forecasting, these figures are overstated.

Although the task force computes a threshold after the fact without knowingly choosing C, we can still back out its implicit choice. Our calculation indicates that its procedures assume that the costs of mispredicting a state failure is $C = 60.2$ times more costly than mispredicting a nonfailure.[20] This value for C would cause a bilateral or international aid agency to "waste" funds on sixty countries not at risk for failure for every one that really is at risk. The task force's given perspective was to use its data and methods to help the U.S. foreign policy establishment narrow its focus and direct foreign aid at a small number of high-risk countries in hopes of making a difference. However, only about three states fail in any one year. As such, $C = 60$ means that the focus of foreign policy would almost not be reduced at all from the list of all countries in the world (191) to $3 \times 60 = 180$. With aid dollars as restricted as they are, this seems like an implausible summary of the political or economic situation and is probably not a useful decision rule.[21]

What is a reasonable summary of the task force's forecasting performance? If $C = 1$, the most commonly used value in other contexts but probably too small here, the task force would correctly classify 0 percent of failures accurately. If C were large enough, they could correctly classify as much as 100 percent of failures, at the cost of mispredicting many nonfailures. Similarly, predicting that states will never fail would have accurately predicted 98.3 percent of all state-years (that is, 100

[20] Esty et al. (fn. 1, 1998) report using a 0.26 cutting point, and they use 0.25 in their new data (which may conceivably indicate that they intended, although failed, to assign $C = 3$) (p. 57). This, by applying equation 26 from King and Zeng (fn. 17), translates to 0.01634 and thus implies that $C = 1/0.01634 - 1 = 60.2$.

[21] Of course, from the perspective of the people in countries at high risk, $C = 60$ might even be too small. A very useful future project would be to survey policymakers to measure their values for C. In all probability, C varies to some extent over people, countries, and time, but there surely are some patterns that would be helpful in evaluating future forecasting efforts.

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percent of nonfailures and 0 percent of failures). In Section IV we use a method of evaluating the performance of forecasting models that works when we are unsure of an appropriate choice for C (or when different people might choose different values).

Forecasting versus Causal Structure

The primary goal of the task force is to make accurate forecasts, but it also draws causal inferences from the same models. Although many researchers seem to think that both goals cannot be accomplished with the same statistical models, this is inaccurate. Indeed, the only way that forecasts can remain accurate far into the future is if the causal structure giving rise to the data remains stable. That means that any claim to accurate forecasts is also implicitly a claim about causal structure. It is true that forecasts are often made using proxy variables (such as infant mortality) and possibly even theoretically uninteresting measures, but, almost by definition, prediction efforts not based in some way on causal structure will fail in the long run when the causal structure inevitably deviates from the convenient measures with which they were once correlated. The classic methodological warning about association not being causation applies equally well to forecasting efforts.

Similarly, almost all causal models that have been specified in international relations implicitly claim that the causal structure being estimated is stable and will remain so for at least some time into the future. Since a finding about a causal structure that changes unpredictably over time is of dubious value, most causal claims imply that accurate forecasts are possible; and, indeed, accurate forecasts are often the most powerful observable implications of the same causal models and can be used as validation for them.

Although there are models that can discern causal structure and in theory are unable to forecast, they are quite unusual. The theory of efficient markets in financial economics is the leading example. In the field of international conflict, Gartzke[22] and Bernstein et al.[23] develop theories which imply that forecasting should be impossible. However, without an explicit theory like this or some knowledge of the kinds of structural breaks that may occur in the future, we must regard models that make causal inferences as also capable of forecasting. If they are not in practice, then their value as causal models must also be ques-

[22] Erik Gartzke, "War Is in the Error Term," *International Organization* 53 (Summer 1999).

[23] Steven Bernstein, Richard Ned Lebow, Janice Gross Stein, and Steven Weber, "God Gave Physics the Easy Problems: Adapting Social Science to an Unpredictable World," *European Journal of International Relations* 6 (March 2000).

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tioned. As such, scholars would do well to judge all models in terms of their forecasting success, regardless of the purpose for which they were originally developed.

In practice, of course, social science research is difficult. Optimally, the world produces a data set to compute forecasts, and new, truly out-of-sample data sets arrive daily that we can use to check the model continually and thereby sequentially improve the forecasts. A large number of out-of-sample tests enable us to rule out random chance and overfitting in accounting for any forecasting success.

Unfortunately, many applications offer only one data set, and in such situations it is difficult to know whether our statistical model is detecting the causal structure or the idiosyncratic features of the particular sample drawn. In the present case the task force had only one data set and tested countless specifications on it. At least according to the task force reports, no out-of-sample tests were conducted. As such, the odds are high that the task force overfit the idiosyncrasies in its data rather than the underlying structure, although it was quite disciplined in keeping to a parsimonious model. Indeed, the task force was careful to explain that its "models are based on historical analysis. It remains to he demonstrated that they will be equally accurate in identifying prospective cases of state failure."[24] Despite these cautionary words, however, academics and policymakers have read the models as making accurate forecasts.

The only way to be reasonably certain about whether its models can forecast would be to wait for more data to come in, but this will take many years, since state failures are rare events. By the time enough data arrive, the international community may miss many opportunities to prevent these disastrous events. In addition, we might also reasonably expect the actual underlying causal structure to have changed to some degree if we wait, and so waiting is not an effective option.

Instead, in our work below we follow standard procedure in forecasting studies and divide the observed sample into two parts, 1955–90 for fitting (or "training") a statistical model and 1991–98 for out-of-sample testing. We use the case-control data for our training set and the entire world for our test set. This means that our out-of-sample test set is a different time period as well as a different set of countries, making it especially difficult. Reserving multiple test sets would have been even better, but the rareness of events (127 in the entire period and only 27 since 1991) makes this infeasible. Overfitting and optimistic assess-

[24] Esty et al. (fn. 2, 1998), 27–38.

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ments are still possible with only one test set, but it is considerably less likely than by evaluating out-of-sample forecasts from in-sample data only. Our choice of the point at which to split the sample between training and test sets is arbitrary, but given the massive changes in the world at about that time, from a cold war to post–cold war international regime, it may be the hardest test available to us. We also report the results of a variety of other stringent tests at the end of Section IV.

Missing Data

Listwise deletion, which the task force uses, is well known to be an inefficient procedure for dealing with missing data (since so many data were discarded). It also biases forecasts and causal inferences unless some implausible assumptions hold.[25] In the task force's final model, one of five observations was discarded. Bias also seems quite likely, since the state-years deleted were not representative of those included. For example, in the global data, 1.68 percent of state-years witnessed failures, but after deleting observations with at least one missing value on their three explanatory variables, this figure rose more than 50 percent to 2.58 percent (even though their dependent variable was fully observed). This would also seem to indicate that valuable information exists in a missingness indicator variable that could be recovered with a better procedure.

The problem of missing data in this application and the effects of listwise deletion on the task force results appear to be more severe than the consequences of dropping a nonrandom 20 percent of state-years from the final model. Listwise deletion also constrained the choice of model. As the authors write: "In many cases, we found that the gaps in the range of particular variables were so great that any possible gains in prediction were offset by statistical uncertainties or missing data problems associated with measuring those additional variables."[26] Because the task force has produced the best collections of data on state failure in existence, this problem results solely from its choice of a statistical procedure for dealing with missing data.

Since valuable information remains in the discarded cases and variables and since we wish to avoid the other problems with listwise deletion, we use multiple imputation[27] to impute the missing values (along

[25] Gary King, James Honaker, Anne Joseph, and Kenneth Scheve, "Analyzing Incomplete Political Science Data: An Alternative Algorithm for Multiple Imputation," *American Political Science Review* 95 (March 2001).

[26] Esty et al. (fn. 1, 1998), 29.

[27] Donald Rubin, *Multiple Imputations for Nonresponse in Surveys* (New York: Wiley Press, 1996); King et al. (fn. 25).

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with software by Honaker, King, Joseph, and Scheve).[28] The idea of multiple imputation is to fill in each of the missing data with several imputed values, creating several completed data sets (where the observed data are identical for all). Then whatever statistical procedure that would have been used in the absence of missing data is applied to each data set, and there is an easy procedure for combining the results from the different data sets. Unlike listwise deletion, this uses all information in the data and appropriately represents uncertainty by filling in the missing values. Since the information to be imputed under our approach is far less than the amount of data discarded under listwise deletion, imputation tends to be more robust. We encourage methodologists to develop multidimensional neural network models for use in imputing missing data, since one could then improve on the missing value techniques used here.

IV. An Improved Forecasting Model

We now discuss the statistical specification for our improved model, evaluate its forecasting performance, and summarize a variety of unusually stringent additional tests we used to ensure against overfitting. In each case, we compare this model with the corrected version of the task force model.

Statistical Specification

We began with the task force's three-variable model and added from their data set a variable we constructed for the *military population* (a logistic transformation of the fraction of the total population of a country in the military). The logic is based on the "resource" (that is, rather than grievance) component of conflict theory:[29] the larger the fraction of the population that has weapons and is trained in military conflict, the more risk there is that internal dissent may lead to state failure.[30]

We also built a *population density* variable (the log of the number of people per square mile relative to the regional median), under the

[28] James Honaker, Anne Joseph, Gary King, and Kenneth Scheve, "Amelia: A Program for Missing Data" (http://gking.harvard .edu, 2000) .

[29] Ted Robert Gurr, "Why Minorities Rebel: A Global Analysis of Communal Mobilization and Conflict since 1945," *International Political Science Review* 14, no. 2 (1993); James B. Rule, *Theories of Civil Violence* (Berkeley: University of California Press, 1988), 178; Mark Lichbach, *The Rebels' Dilemma* (Ann Arbor: University of Michigan Press, 1995), 4–6.

[30] See also John D. McCarthy and Mayer N. Zald, "Resource Mobilization and Social Movements: A Partial Theory," *American Journal of Sociology* 82 (May 1977); Charles Tilly, *From Mobilization to Revolution* (New York: McGraw-Hill, 1978).

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simple theory that internal conflict requires people to be near others who might disagree. Population density is a very crude indicator of physical proximity, which, as Lichbach[31] (and the many references therein) explains, should reduce collective action costs by making the communication of grievances easier and by allowing for repeated interactions and therefore more trust. Collier[32] is also interested in population density but finds the opposite result. Fearon and Laitin[33] find results that support the theory they describe as not robust to specification decisions.

As a measure of the institutionalization of democratic institutions, we also included the task force's measure of *legislative effectiveness* (qualitatively coded as none, largely ineffective, partly effective, and effective). Przeworski et al.[34] argue that parliamentary institutions make a democracy more likely to endure. Legislative effectiveness is an important component of democratization, but it is sufficiently distinctive and divergent from the other components that we control for it separately.

Like the task force's variables, our additions are reasonable choices and widely discussed in the qualitative literature as possible risk factors, but they are not derived from anything approaching an empirically verified formal theoretical model. A sufficiently convincing story can be told about the theoretical expectations for each of these six variables (seven, if we count the two democracy dummies separately), but instead of pretending that our "hypotheses" were constructed ex ante, we prefer to recognize this as an exploratory analysis. Our more modest goal for this stage (that is, in addition to the more difficult goal of producing reliable forecasts) is to identify empirical regularities that may help in building theories rather than to test an existing fully specified theory.

This line of work is still quite valuable from a theoretical perspective by virtue of the substantial evidence it provides against all theories that assign a role to any variable other than the six in the present analysis (from the task force's original set of 1,231). The qualitative literature on the causes of state failure and its various components is far richer than is summarized in these six variables, but unless some case can be made

[31] Lichbach (fn. 29), 158–65.

[32] Paul Collier, "Economic Causes of Civil Conflict and Their Implications for Policy," in Chester A. Crocker, Fen Osler Hampson, and Pamela Aall, eds., *Managing Global Chaos* (Washington, D.C.: U.S. Institute of Peace, 2000), 6.

[33] James Fearon and David Laitin, "Weak States, Rough Terrain, and Large Scale Ethnic Violence since 1945" (Paper presented at the annual meeting of the American Political Science Association, Atlanta, 1999).

[34] Adam Przeworski, Michael Alvarez, J. A. Cheibub, and F. Limongi, "What Makes Democracies Endure," *Journal of Democracy* 7 (January 1996).

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that the largest state failure data set ever constructed excludes variables identified in these theories, these theories can be regarded as inconsistent with the data and should be rejected.

Finally, our prior work suggests that we should expect massive interactions and nonlinearities, just as in international conflict data,[35] in part since the effects of the explanatory variables are expected to differ over types of countries and regions and because of the heterogenous definition given for state failure. In contrast, assuming that all or most interactions are absent, as most scholars do (and as the task force did) when they use logit models even with some interactions, is a heroic assumption. The "curse of dimensionality" ensures that the six-dimensional space represented by all the linear and nonlinear interactions of our six explanatory variables, plus the possible nonlinearities in the main effects, is almost incomprehensibly immense.[36] Since no accepted theory can rule them out, and few theories have even addressed the issue, we prefer not to assume knowledge of these interactions, beyond some smoothness in the functional forms, and instead introduce a model capable of estimating what it can from these data. We then use extensive out-of-sample tests (described below) to protect against being fooled by overfitting. We therefore follow this rule when feasible: *when we know something, we assume it; when we don't know, we estimate it.* As long as the estimation process is scientifically disciplined, this approach is superior to making draconian assumptions without empirical knowledge.

We impute the missing data and then use the neural network statistical model described in Beck, King, and Zeng,[37] modified by what are known as "committee methods." A neural network model is parametric and is just like a logistic regression analysis except that the functional form can take on many shapes in addition to the logit model's escalator-

[35] Nathaniel Beck, Gary King, and Langche Zeng, "Improving Quantitative Studies of International Conflict: A Conjecture," *American Political Science Review* 94 (March 2000).

[36] To understand the curse of dimensionality in this context, consider a regression with one continuous dependent variable and a single ten-category explanatory variable. To estimate this regression without assumptions, we need to estimate ten quantities, the mean of Y within each of the ten categories of X (e.g., the mean starting salary for people with each of ten levels of education). We could easily do this if we had, say, a sample of one hundred observations within each of the ten categories. By contrast, linear regression would summarize these ten numbers with only two, a slope and a constant term, by making the assumption that nothing is being lost. Now suppose we added one more ten-category explanatory variable. The curse of dimensionality is that we need to multiply not add—to estimate one hundred quantities, not merely twenty (graphically, we move from a bar chart to a checkerboard where the height of each square represents dollars of starting salary). An analysis with, say, ten ten-category explanatory variables requires the estimation of ten billion quantities, and summarizing that with a linear regression that has only eleven parameters and maybe even a few (linear) interaction terms is a stunningly strong assumption.

[37] Beck, King, and Zeng (fn. 35).

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shaped curve (ignoring the steps!), depending on what the data suggest. We summarize these models in Appendix 2. The idea of committee methods is to run a number of neural network models, varying the number of hidden neurons and prior weights (which together indicate how much smoothness we expect in the functional form). The individual models are then combined, either by weighting them according to some estimate of performance or by simple averaging. Much empirical and some theoretical work indicates that simple averaging, which we use, usually works better because in-sample estimates of performance tend to be highly variable. Bishop[38] proves that committee methods improve out-of-sample performance through variance reduction. Committee methods remove some of the arbitrariness that accompanies the real use of most statistical methods that require choosing one model specification out of a large potential set.[39]

Evaluating Forecasting Performance

We now provide evidence that our new model forecasts better than the task force model, regardless of the costs one assigns to the two types of misclassification. To do this, we use a *Receiver-Operating Characteristic* (ROC) curve.[40] (This obscure terminology comes from signal processing theory, where the receiver [a decision maker in our framework] must decide whether each item in a string of noisy binary data is really a 0 or a 1.) The ROC graph thus has the fraction of 0s correctly predicted plotted vertically and the fraction of 1s corrected predicted plotted horizontally. The key point is that for any value of C, a model and data will produce only *one* pair of numbers for the percentage of failures correctly predicted and the percentage of nonfailures correctly predicted. This one pair of numbers appears as *one point* in an ROC graph, such as that in Figure 1. Changing C a little at a time over its entire possible range and plotting the corresponding pairs of percentages correctly predicted values on the graph give the complete ROC curve.

[38] Christopher M. Bishop, *Neural Networks for Pattern Recognition* (Oxford: Oxford University Press, 1995), 366.

[39] All members of the committee that constituted our model were based on the same input variables and three numbers: a random number seed for the starting values (which we include here to make it easier to replicate our results), the number of hidden neurons, and the prior standard deviation for the weights. The triples for the members of our committee are 45,3,1; 8,3,2; 908,3,3; 85,3,5; 908,4,2; 35,5,1; 12345,5,5; 768,5,6; 134,5,10; 8,7,3; 9,8,5; 45,8,6; 923,10,1. In general these are all fairly smooth neural network models. We chose this set based on our experience in fitting analyses to similar data and through some preliminary analyses. We expect models that can forecast even better could be developed.

[40] See D. M. Green and J. A. Swets, *Signal Detection Theory and Psychophysics*, rev. ed. (Huntington, N.Y.: Krieger, 1974); C. E. Metz, "Basic Principles of ROC Analysis," *Seminars in Nuclear Medicine* 8 (Spring 1978).

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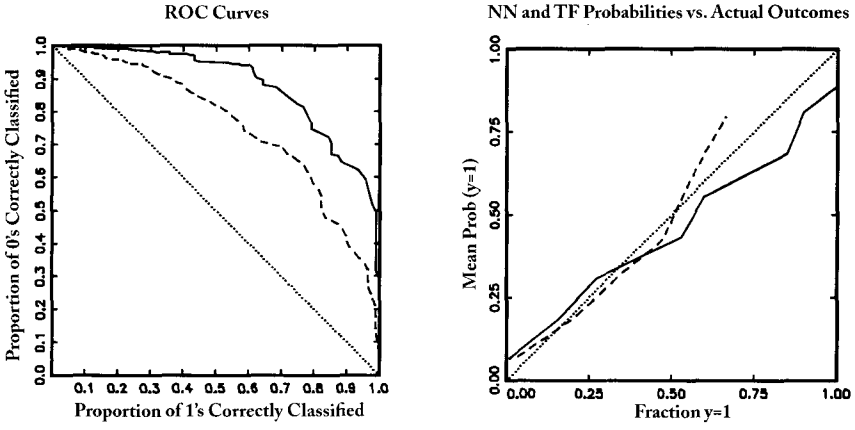

FIGURE 1
IN-SAMPLE CASE-CONTROL MODEL FIT[a]

[a]Graph a is the ROC curve, which evaluates comparative model performance, and graph b evaluates the veracity of the estimated probabilities. In both graphs the dashed line refers to the task force model and the solid line is from the neural network model.

We offer an example of the ROC curve in the left graph in Figure 1 and now discuss how to interpret it. For reference, the diagonal line in this graph is what the ROC curve would be if probability values were selected randomly (from a uniform distribution) and unrelated to the data. The top right point in the graph indicates 100 percent of 1s and 0s correctly predicted. Thus, the farther above the diagonal line a curve drawn on the basis of an empirical model falls, the better is the model performance. On the basis of this type of analysis, a researcher would conclude that one model dominates another if its curve is greater than the other model for every point, that is, for every possible cost of misclassification. In situations where a model performs better in some areas and worse in others, we may need to narrow the range of possible values for C in order to choose one model over the other. Out of sample, however, we do not know until after the fact where we are on the curve, that is, what are the percentages of 0s and 1s correctly predicted.

In graph a of Figure 1, the dashed line is computed from the task force model estimated with the in-sample case-control data from 1955 to 1990 and evaluated in the same data (which makes it a measure of fit, not of forecasting ability). In addition, the solid line in the graph gives the ROC line from our neural network model. Since this solid line is always above the dashed line, the neural network model dominates

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the task force model, no matter what normative decision one might make about the costs of misclassification, *C*. Of course, since this graph is both fit and evaluated in the same data, it indicates that the neural network model fits the data better, not that it necessarily forecasts better.

Before moving to an evaluation of out-of-sample performance, we also offer a test of the veracity of the estimated probabilities. A probability that is accurate gives the fraction of times a state with the given characteristics will actually fail. To evaluate these probabilities, we sort estimated probabilities into bins of 0.1 width: [0,0.1), [0.1,0.2),...,[0.9,1]. For observations falling in each bin, we compute the mean predicted probability from the model (which will often be somewhere near the midpoint of each interval), as well as the observed fraction of 1s. If model probabilities are accurate, these two quantities should be close: for example, if the probability of failure is forecast to be 0.2 for a group of states, then about 20 percent of these states should actually fail. We then compare the two in graph b in Figure 1 to check the fit of the model in the training set (and below to evaluate the forecasts in the test set). In this figure both models are fairly close to the 45-degree line, indicating fairly accurate in-sample probabilities. The graph reveals the neural network model to have more informative probabilities (higher values), although these appear to be slightly underestimated (perhaps suggesting that the neural network priors should be adjusted to allow somewhat less smoothness, although we do not follow up on this minor point).

We now consider the more important out-of-sample performance of both models. Figure 2 gives analogous graphs, estimated from the 1955–90 data and evaluated in the 1991–98 data. When we refer to the "task force" model in these graphs, we make the case-control corrections described in Section III (otherwise, the probabilities in graph b would be far worse).

As can be clearly seen, in the ROC graph a, the (solid) line for the neural network model is always above the (dashed) line representing task force model. Thus, for every value of *C*, the neural network model has a higher percentage of 1s correctly predicted and a higher percentage of 0s correctly predicted out of sample. Whatever one's normative preferences, therefore, the neural network model is superior to the (prior-corrected) task force model.

Graph b in Figure 2 indicates that the neural network probabilities are both more accurate (closer to the 45-degree line) and more informative than those for the task force model (because they extend farther

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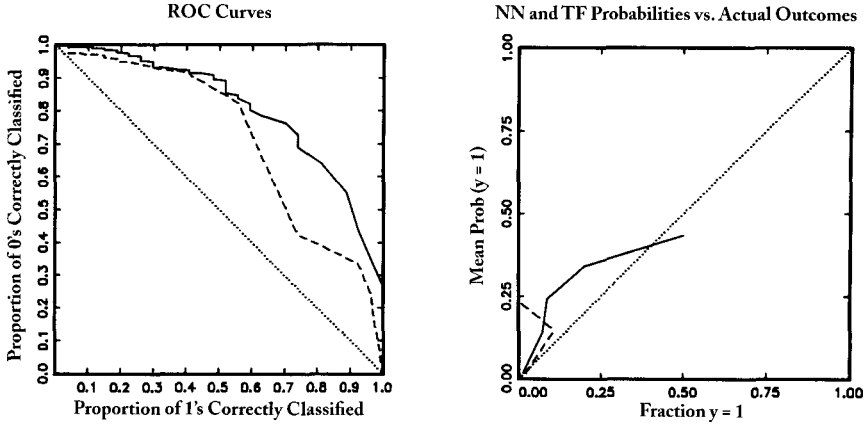

FIGURE 2
OUT-OF-SAMPLE GLOBAL MODEL FORECAST[a]

[a]Graph a is the ROC curve, which evaluates comparative model performance, and graph b evaluates the veracity of the estimated probabilities. In both graphs the dashed line refers to the task force model (although we also prior corrected, as in Section III) and the solid line is from the neural network model.

up the diagonal). The task force figures here are prior corrected, which is why they do not extend as high as in Figure 1 and why they are anywhere near the 45-degree line. Unfortunately, even with the correction, they do fairly poorly and are not accurate. Indeed, even prior correction is insufficient, since the out-of-sample ranking of states in the probability of failure is not preserved in the task force model. The dashed line doubling back on itself means that higher estimated task force probabilities actually correspond to lower actual rates of state failure. The solid line, representing our neural network analysis, is not perfect, but it indicates that our estimated probabilities are at least monotonically related to actual instances of state failure and are usually fairly close to the diagonal equality line. Taken together with the ROC graph, the available evidence indicates that our neural network committee model offers out-of-sample forecasts that are better than the (prior-corrected) predictions of the State Failure Task Force.[41]

[41] Each of the methodological improvements we made to the task force model improved results over the same model without that feature, and all were necessary to generate a model that dominated the (prior-corrected) task force model for any value of C. Of course, prior correction alone was sufficient to improve a great deal on the original task force analysis. A rough ranking from most to least important in changing the results is prior correction, neural networks, committee methods, the additional covariates, and multiple imputation for missing data.

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Additional Tests to Ensure against Overfitting

We conducted several additional tests to verify our claims to have built a model that can forecast more accurately and to have picked up some piece of the underlying structure. In addition, we make these tests somewhat more difficult by using various subsets of our case-control data for our training sets and subsets of the global data for our test set. Taking all this into account, we can think of no political science modeling exercise that has applied more stringent tests, whether for the purpose of forecasting or for estimating causal effects.[42]

For our first test, we divided the country-years randomly between test and training sets and examined the ROC and probability graphs, as in Figures 1 and 2. In all cases, aside from what would be expected due to random error, our neural network committee approach dominated the (prior-corrected) task force model. We also computed the marginal effect graphs we report in Section VI and found that the causal structure uncovered stays quite stable across the different random subsets.

We also use what we call the Stanford Test (so named because several Stanford faculty and fellows suggested it at a talk we gave there on a related subject), which combines a simple version of cross-validation with out-of-sample verification. First, all countries are randomly divided into two groups, which we label A and B. The training and test sets are defined by dividing the sample chronologically at 1990, as before. Then country group A in the training set is used to forecast country group B in the test set. Similarly, country group B in the training set is used to forecast country group A in the test set. Hence, the forecast is both out of sample (to a future time) and out of space (to different countries). This is one of the hardest (reasonable) tests that can be constructed to ascertain whether a statistical model has uncovered a stable, causal structure and has not overfit the idiosyncrasies in the data that do not persist. Although difficult, the Stanford Test is reasonable to apply to any analysis aimed at uncovering lawlike causal statements or making genuine policy-relevant forecasts. (Although we do not pursue the possibility beyond the use of the test here, the procedure could be profitably generalized to all possible subsets A and B and formalized to yield sampling probabilities.)

In the present case our test was made more difficult by the fact that there are only twenty-seven events in our test set after 1990 and thus only about half that in country groups A and B. But even with the sam-

[42] We summarize the results of these tests here, rather than presenting detailed accompanying figures, since this would involve including numerous figures for each one presented in this paper.

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pling error induced by using only half the data at a time, the forecasting performance, as judged by the ROC and probability graphs, and the causal structure, as judged by our marginal effect plots, were all quite consistent with one another. In virtually all cases our neural network committee approach dominated the (prior-corrected) task force model.

We also examined whether the substantive variables we measured, along with the estimated functional form, were sufficient to pick up the information represented in the country labels. That is, in almost any country-level analysis, a set of regional dummy variables will correlate highly with the outcome variable. The key is determining whether one's substantive variables pick up that variation, making the ad hoc idiosyncrasies of including dummy variables unnecessary. In our case we compared the ROC curve for our model with one where we also added a set of regional dummy variables. Predictably, the model fit the in-sample (training set) much better. However, the key is that the out-of-sample forecasts to our test set were considerably worse. This indicates that, indeed, we were able to "get rid of proper nouns":[43] the dummy variables are unnecessary and so our substantive variables apparently do not exclude any important structural components that correlate with the country names.

We also tried several measures of economic growth, because the literature at least since Huntington[44] has suggested that growth might lead to state failure by empowering middle classes in authoritarian regimes. Unfortunately, we were unable to find evidence in support of this hypothesis or evidence that growth in any way adds forecasting power to our models. We also tried adding the number of years since the last state failure, to model time-series dependence in the data,[45] but we found no evidence to support this variable either, although some of this effect might be represented in existing variables. In addition, we tried a time trend, but like the regional dummies it helped fit the in-sample data better but forecast considerably worse. We also examined many other individual variables from the task force data set, but finding no evidence that they could help in forecasting or would alter our substantive conclusions, we excluded all of them.

[43] Adam Przeworski and Henry Teune, *The Logic of Comparative Social Inquiry* (Malabar, Fla.: Krieger, 1982).

[44] Samuel P. Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968).

[45] Nathaniel Beck, Jonathan Katz, and Richard Tucker, "Taking Time Seriously: Time-Series-Cross-Section Analysis with a Binary Dependent Variable," *American Journal of Political Science* 42 (October 1998).

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The results of these successful validations from test sets defined on the basis of time, random assignment, the Stanford Test, the regional dummies, and other substantive variables cause us to be more confident about our ability to forecast and to believe we have found something approximating causal structure. We could still be wrong, and past performance is no guarantee of future success; but it seems hard to argue that these tests would not increase the chances that our model would hold for new data not yet collected. We think that future research by the task force and others in this and related fields would benefit from applying these procedures.

V. Sample Forecasts

Studying individual forecast probabilities of state failure must be done carefully, following the ideas about decision theory presented in Section IV. A key issue is that a probability that may seem high from one perspective could easily be considered low from another.

Our individual country-level forecasts easily distinguish countries that obviously do not have much risk of failure from those with some risk, something that previous literature has not accomplished. For example, the published task force numbers—the only figures that have been used by policymakers—include a forecast that France would fail in 1960 with an incredibly high 0.29 probability. In contrast, our model forecasts a probability of state failure in France of nearly 0 for almost all years. More difficult is distinguishing among the countries with some risk of failure. For example, with information available in 1964, our model gave a 0.35 probability of Uganda failing in 1966 (which is very high for the probability of failure occurring in just one year). Uganda failed that year. By contrast, our model gave a forecast probability of Kenya failing in that year of less than a third of the probability for Uganda (Kenya did not fail).

Of course, these examples are anecdotes culled from the thousands of predictions that come from our model and that cannot be fully presented in this short article. Instead, we now summarize the numbers by giving, in Table 4, our best and worst forecasts for all post-1990 countries, based on the data from the case-control subset of countries up to 1990. (Thus, our methods could be used to compute much better two-year-ahead forecasts than these by using all the information available; the 1998 forecasts, for example, are based on estimates from more than eight years earlier.) For example, the ten country-years in the top left section of the table are those with the highest forecast probabilities of

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TABLE 4
BEST AND WORST PREDICTIONS

Best Predictions			*Worst Predictions*		
1. Highest Prob (Failure), Failure Observed			*1. Lowest Prob (Failure), Failure Observed*		
Country	*Year*	*Prob.*	*Country*	*Year*	*Prob.*
Senegal	1991	.5307	Mexico	1994	.0137
Kyrgyzstan	1995	.4563	Bosnia-Herzegovina	1992	.0174
Kazakhstan	1995	.4147	Algeria	1991	.0259
Cambodia	1997	.4122	Sierra Leone	1991	.0312
Georgia	1998	.3913	Congo-Kinshasa	1992	.0333
Armenia	1994	.3470	The Gambia	1994	.0405
Guinea-Bissau	1998	.3214	Haiti	1991	.0592
Thailand	1991	.2787	Lesotho	1994	.0627
Zambia	1996	.2348	Tajikistan	1992	.0667
Georgia	1991	.2285	Indonesia	1997	.0781
2. Lowest Prob (Failure), Nonfailure Observed			*2. Highest Prob (Failure), Nonfailure Observed*		
Country	*Year*	*Prob.*	*Country*	*Year*	*Prob.*
Finland	1992	.0014	Peru	1998	.5955
Sweden	1994	.0015	Bangladesh	1996	.4841
Estonia	1997	.0015	Kyrgyzstan	1993	.4781
Finland	1993	.0016	Kyrgyzstan	1994	.4752
Switzerland	1993	.0016	St. Kitts-Nevis	1997	.4570
Switzerland	1994	.0016	Uzbekistan	1991	.4394
Norway	1993	.0017	Bangladesh	1995	.4355
United Kingdom	1994	.0017	Kyrgyzstan	1997	.4187
United Kingdom	1996	.0017	Guinea-Bissau	1996	.4110
United Kingdom	1991	.0017	San Marino	1998	.4006

state failure among post-1990 country-years that experienced actual state failures. Only one of these probabilities is greater than 0.5, and so we would not have necessarily expected any one of the others to fail in the year predicted, unless the costs of misclassifying state failure were worse than the costs of misclassifying nonfailure. However, the results clearly indicate that the set of countries should have produced a number of state failures and so, for policy purposes, these would presumably have been watched carefully. That is, our methods would have made it possible to know that the probability of state failure was very high in each of these cases and likely to occur in the disturbingly high fraction of cases indicated by the given probability.

For another example, our worst (that is, highest probability) predic-

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tions among states that did not fail post-1990 are given in the bottom right of Table 4. If policy analysts had used our methods, they would have expected to see some state failures among this group. Of course, the high probabilities without observed failures do not necessarily indicate a problem with our model, since the probabilities overall are accurate (that is, 30 percent of states with 0.3 probabilities really do fail 30 percent of the time). If they were accurate for these countries, nonfailure would be perfectly consistent with the model's predictions for any *one* country at any one time, although for sets of countries the probabilities ought to be realized in actual failures as predicted.

VI. Exploring Empirical Regularities

In this section we discuss a variety of empirical regularities about state failure uncovered by our analyses. These regularities are descriptive features of the underlying causal structure. The tests given in Section IV indicate that these empirical regularities are stable and predictable features of the world and account for some of our success at forecasting. They are not necessarily equivalent to causal effects, which require additional assumptions about counterfactuals, the validity of which neither we nor the task force explores in any detail. For example, that people with more education make more money is an empirical regularity. The claim that any one person, or people on average, *would have* made more money if, ceteris paribus, they had received more education is a causal claim. Causal claims are more difficult to substantiate because they involve counterfactuals for which no direct evidence exists.[46] Although different from causal effects, empirical regularities are still very valuable components of knowledge, since any theories of state failure would need to be consistent with them. Similarly, any causal story would need to account for these verified facts about the world.

Our procedure for summarizing the empirical regularities involves using marginal effect graphs—plots of the probability of state failure by one explanatory variable (at a time), while holding constant the set of control variables at different values to see how the relationships change. In addition, each marginal effect graph itself portrays an interaction between democracy and another variable.

Figure 3 presents some of these marginal effect graphs. In each graph the predicted probability of state failure, computed from our neural network committee model, is plotted vertically. One of the explanatory variables is plotted horizontally in each graph, with three

[46] See King, Keohane, and Verba (fn. 11).

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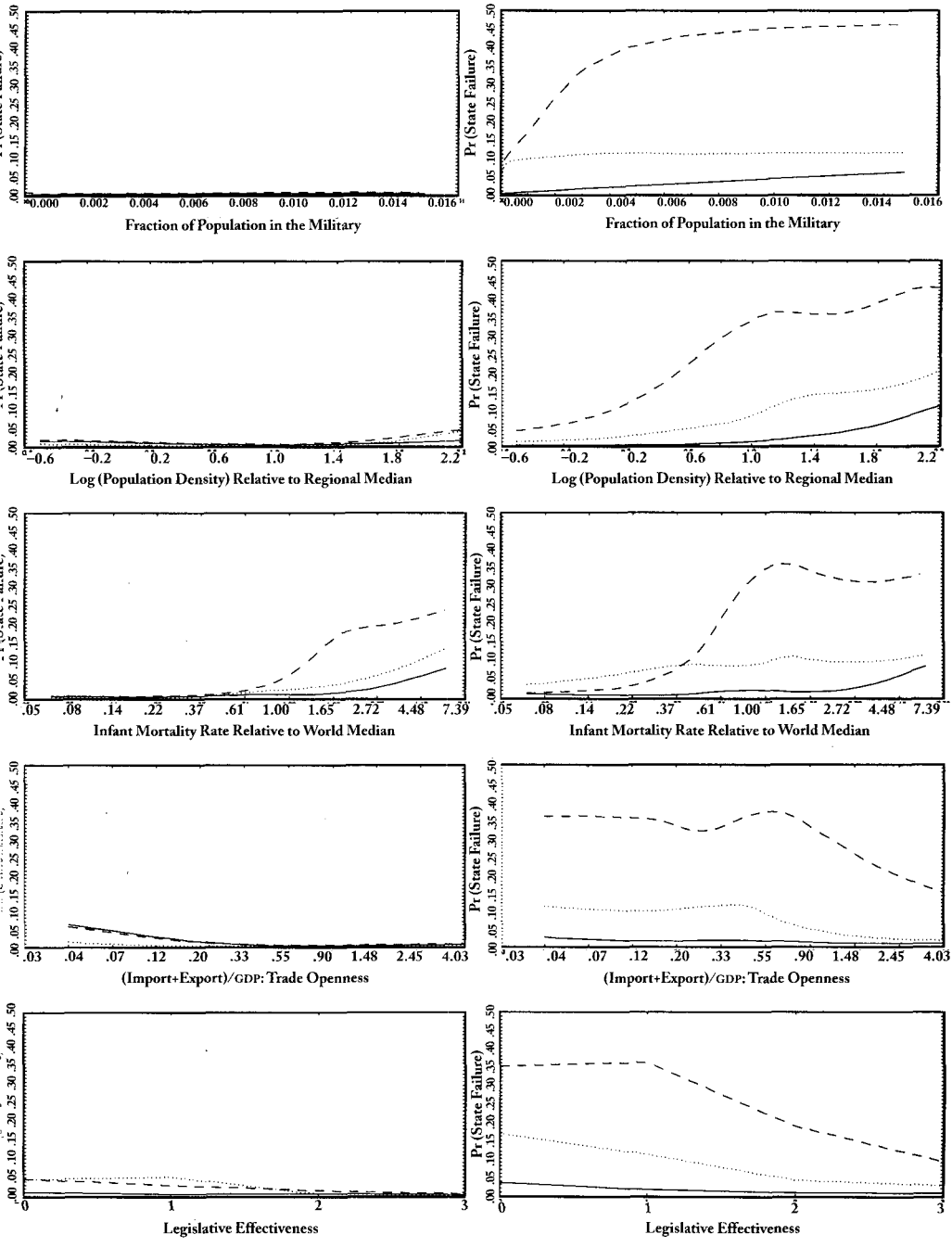

FIGURE 3

MARGINAL EFFECT PLOTS[a]

[a]Each plot gives the predicted probability of state failure vertically by one of the explanatory variables horizontally. Variables not named in each graph are held constant at the medians of the G7 countries for the left column and for state-years with failures for the right column. In each graph, the solid line is autocratic states, the dotted line is full democracies, and the dashed line includes partial democracies.

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lines drawn corresponding to full democracies (dotted), partial democracies (dashed), and autocracies (solid). The first column holds control variables (those not named on each graph) constant at the median value of the G7 countries (except for the U.S., which is not included in the task force data). The second column holds constant control variables at the median for all countries with state failures. The graphs with controls held constant at the global median are similar to those in the second column, and so we do not present these. In addition, although in general with flexible models like neural networks holding constant, the control values at even slightly different values can result in very different looking marginal effect plots, such is less likely to be the case with our models since our priors impose smoothness on the functional form. (We exclude confidence intervals from these graphs for visual clarity, but all are sufficiently narrow to make the general interpretations from these lines reliable.)

The striking difference between the first and second columns of this graph shows that our model picks up the obvious differences between major industrial democracies and other countries as interactions among substantive variables, rather than as indicator variables with country labels. Of all the variables examined in the first column, only very high levels of infant mortality and, to some degree, very low levels of trade openness and ineffective legislatures increase the probability of state failure to any significant level. The infant mortality result especially is fairly striking: only states with governments that are sufficiently competent to keep infant mortality below the global median have comparatively low probabilities of state failure; other countries, even when they are alike in all other measured respects to the G7, have substantial probabilities of failure (as high as 0.25).

The second column represents time effects for countries that have values of their control variables like the median of those with state failures. The fact that these graphs are so similar to ones drawn on the basis of the global medians (not shown) indicates the stability and general applicability of the large effects witnessed there. Apparently, the main differences in the size and nature of the effects across the sample of countries is between the G7 and everywhere else. An obvious effect outside the G7 is the similar effect of democracy: for most of the ranges of all of the variables, partial democracies have a higher risk of failure than either full democracies or autocracies. Most often, autocracies have even lower risks than full democracies, although the differences here are mostly very small. We now interpret this effect along with that for legislative effectiveness in the bottom right corner of the figure. The

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story is similar to accounts such as Hibbs, Gurr, and Muller and Weede;[47] what our data add to these is the qualification that it does not apply to countries when holding constant other variables at the level of the G7. That is, it is not only that the G7 countries are different or that they have some unmeasured characteristics that account for this difference; it is instead that when the control variables take on values similar to those in the G7, even for non-G7 countries, this usual story about democracy is greatly reduced.

One version of this story is as follows.[48] Autocracies are countries that have repressive central governments. Since dissent is nearly impossible, little conflict of any kind, much less state failure, is observed. Such differences between the government and the people as may exist are not expressed. In contrast, in partial democracies differences between the people and the government are publicly revealed to some degree. However, because the institutions of democracy in partial democracies are not capable of adjusting government policy quickly enough, public attitudes can move far from the position of the government and stay there for a sustained period of time. In some cases, when the government cannot control this public conflict, the risk of state failure increases. We measure finer gradations of degrees of democracy in the present framework with the legislature effectiveness measure, which indicates the degree to which the legislature is channeling political dissent through electoral politics. The more this process is institutionalized, the lower the risks of state failure. Countries that are classified as full democracies typically have high legislative effectiveness as well as many of the other related attributes. Since public attitudes and government policy in these countries cannot stray far from one another for long, the risks of state failure drop to low levels, even though there are exceptions. (Clearly this is a very different story from that told by the results and arguments of the democratic peace literature, where the more democracy the better the chances of reducing conflict between nations.)[49]

[47] Douglas A. Hibbs Jr., *Mass Political Violence: A Cross-National Causal Analysis* (New York: Wiley, 1973); Ted Robert Gurr, "Persistence and Change in Political Systems, 1800–1971," *American Political Science Review* 68 (December 1974); Edward N. Muller and Erich Weede, "Cross-National Variation in Political Violence: A Rational Action Approach," *Journal of Conflict Resolution* 34 (December 1990).

[48] See, for example, Huntington (fn. 44); Ted Robert Gurr, "Why Minorities Rebel: A Global Analysis of Communal Mobilization and Conflict since 1945," *International Political Science Review* 14, no. 2 (1993); R. J. Rummell, "Democracy, Power, Genocide, and Mass Murder," *Journal of Conflict Resolution* 39 (March 1995).

[49] See also Edward Mansfield and Jack Snyder, "Democratization and War," *Foreign Affairs* 74 (1995).

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Just as having a gun in a household is a risk factor for murder and suicide, the first graph in the second column of Figure 3 indicates that larger military populations are a risk factor for state failure. It is not clear whether this is because governments that fear failure increase the size of their military in anticipation or whether larger fractions of the population belonging to the military means that there are more potential dissenters with access to weapons. Either way (and in all likelihood, both factors are operating), this result may provide additional motivation for the ongoing movement of the international community toward concepts of "human security."[50] Note also that most of the conditional effect of this variable occurs when the military fraction of the population is relatively small, with the effect still increasing but at a much slower rate thereafter. Although this may suggest strategies for designing aid efforts that can achieve the largest effect for each dollar spent, further research would be required first.

The second row of the second column of Figure 3 shows that the risk of failure increases gradually with population density. This is a fairly conventional result: people need to interact before they start fighting, but it could of course be some other characteristic of densely populated nations that is operating.

The third row illustrates the effects of infant mortality in partial democracies sharply increasing the risks of state failure and then leveling off. Infant mortality is a often a good proxy for the competence of the state and it clearly picks this up here. When the infant mortality rate is equal to the world median or worse (a value of 1 or greater on the horizontal axis), the marginal probability of state failure is approximately constant. However, in those countries where infant mortality is lower (to the left of 1)—and this is often a direct result of governmental intervention—the probability of state failure drops precipitously. Infant mortality is lower in most Western democracies, but there are numerous exceptions, most of which support the role of this variable as a measure of state competence.[51]

[50] For example, Lincoln C. Chen, "Human Security: Concepts and Approaches," in T. Matsumae and Lincoln C. Chen, eds., *Common Security in Asia: New Concepts of Human Security* (Tokyo: Tokyo University Press, 1995); Gary King and Christopher Murray, "Rethinking Human Security," *Political Science Quarterly* (forthcoming), preprint available at http://gking.harvard.edu.

[51] For example, infant mortality in Cuba, where the government is so involved that the minister of health chairs a separate meeting to investigate the case of each infant who dies in the country, is lower even than the U.S. Some exceptions would include cases where natural disasters overwhelm the resources of even the most conscientious governments. Other exceptions include states where targeted interventions, such as by the World Health Organization, reduce infant mortality without as much help from the government involved (Ghana may be an example of this).

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The task force's choice of a trade openness variable reflects the optimistic hypothesis that foreign policies designed to open markets might also reduce the incidence of state failure. We have found little direct support for this idea in the literature, but a number of related variables are relevant. For example, O'Kane[52] uses export specialization (the percentage of total export revenue derived from the largest single source in a country) to predict the probability of a coup. Her theory is that overspecialization leads to income instability, which in turn causes frustration, low levels of development, and reduced governmental legitimacy.[53] Export specialization is correlated with trade openness since countries that derive most of their GDP from a single export also receive most of their consumer goods and a lot of their food from abroad and so have high scores on the trade openness variable. This is consistent with our results, to which we now turn.

We find that high levels of trade openness are associated with the probability of state failure, but with virtually all of the effect occurring in nations already having extremely high degrees of openness. This nonlinear trade openness result reveals significant biases in the task force's logit results in Table 1: over most of the range there is no relationship between trade openness and state failure. What the logit model is picking up (and incorrectly attributing to the entire range) is the effect of outlier countries, the lower probability of state failure in countries with larger trading businesses than local economies (that is, when the horizontal axis is to the right of 1). These otherwise unexpectedly wealthy countries include the oil-exporting states and states like Singapore that occupy the "middleman" trading positions.[54]

We reemphasize that these empirical regularities are marginal effect plots; they are obviously not the results of randomized experiments or of an observational study where we know the explanatory variables are exogenous. They are "partial" (or "direct") causal effects that control for some of the consequences of the causal variables being studied. Our tests indicate that stable structures underlie these data, no matter how we look at them, but whether these should be relied on to draw causal inferences or change foreign aid policy must be the subject of additional study. Since foreign aid decisions are easier to identify and their moti-

[52] Rosemary O'Kane, *The Likelihood of Coups* (Averbury: Aldershot, 1987).

[53] For a contrary view, see Collier (fn. 32).

[54] It is also worth noting that this result unfortunately blurs the distinction between variable-based explanations, which we prefer, and country-based "proper noun" stories. More generally, this result also suggests, as is the case, that logistic regression results are more sensitive to outliers than are neural networks. Whereas an outlier can throw off the entire logistic curve, neural networks, by contrast, will usually map outliers separately and localize their effects to small pieces of the functional form.

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vations could be (and have been) the subject of much analysis, we have a chance of ascertaining where foreign aid dollars would have their largest effects.

VII. Concluding Remarks

Although we find problems in the work of the State Failure Task Force that lead to overly large forecasts, exaggerated assessments of forecasting performance, and biased causal inferences, we wish to emphasize that it has nonetheless accomplished a great deal. The members have merged their deep knowledge of state failure to create a remarkable, carefully documented data set. The many millions of dollars invested in creating these data far exceed the resources spent on any other data set on state failure, indeed, on almost all other data sets in the discipline. The data set codifies numerous qualitative insights and knowledge from a diverse variety of area studies and other experts brought in to add their expertise to individual variables. The result is that it is now possible to test numerous theories systematically, many for the first time. Even their favored model, when appropriately corrected, performs quite well. We were able to outperform even corrected versions of their forecasting model, but only by introducing new methods to build on what the task force had already produced.

We have no doubt that further work in data, methodological innovation, theory, or area studies would produce better forecasts than even those offered here. We hope this line of research also helps the academic community and policymakers to understand, forecast, and address the critical problem of state failure.

As discussed in the introduction, the task force's definition of state failure is highly heterogeneous, and so progress would likely be made by breaking down this variable into its component parts. We conclude here with a related issue and suggestion for further research. Although all forecasts reported here are "real" in the sense of being tested out of sample, with data only available at least two years prior to the potential event, the logical status of each of the variables in the task force definitions is not entirely what was intended. In some sense, the explanatory variables (infant mortality, partial democracy, legislative [in]effectiveness, and so on) are really indirect indicators that the state has *already* failed, whereas their heterogeneous dependent variables (genocide, disruptive regime transitions, and revolutionary wars aimed at displacing the regime) are not really measures of state failure but instead are indicators of some of the disastrous *consequences* of state failure. Studies in-

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tending to forecast and explain the consequences of state failure are obviously important,[55] but so would be a study that tried to predict and explain the *onset* of state failure—the collapse of the central authority structures of the state. To do this would require a different strategy for data collection than that pursued by the task force and a more tailored, operational definition of state failure and the institutionalization, legitimacy, and authority of the state.

Appendix 1: State Failure Data

The task force data set we received includes 1,231 variables for 195 distinct countries between 1955 and 1998. Compiled from many sources, the data set provides information on political, sociological, economic, cultural, religious, educational, demographic, environmental, and public health characteristics of each country with a population greater than half a million. The data set contains information from commercial sources and from sources in the public domain, and some of the data were created or corrected by the task force; none of the data were classified.

While the task force has made older versions of its dependent variable available on the web, it has until now prevented others from accessing the rest of its extensive data collection and (for parts) has not indicated from which commercial source it can be purchased and precisely how to merge the different sources to re-create their data set. Upon publication of this article, the full task force data set to which we had access will be available at http://gking.harvard.edu. The task force continues to update and improve its data, and we encourage it to release subsequent versions so that the scholarly community may continue to benefit from its efforts and further improve forecasts of state failure.

This appendix lists some of the representative variables for interested readers. Due to space limitations, it is impossible to give detailed information on the variables (for example, measurement) here, and the original data set should be consulted for that purpose. A complete list of data sources appears in the task force report.[56]

The data set includes such variables as country code/name, year, geographic region, case-control indicators; information on leaders (name, education level/discipline, etc.); characteristics of the ruling elite; ethnic/

[55] See Robert Melson, *Revolution and Genocide: The Origins of the Armenian Genocide and the Holocaust* (Chicago: University of Chicago Press, 1992); Matt Krain, "State Sponsored Mass Murder: The Onset and Severity of Genocides and Politicides," *Journal of Conflict Resolution* 41 (June 1997); Benjamin Valentino, "Final Solutions: The Causes of Genocide and Mass Killing," *Security Studies* 9 (Spring 2000).

[56] Esty et al. (fn. 1, 1998).

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linguistic/religious groups; population (sizes, composition, density); income, GDP/GNP (various measures), consumption, government expenditures, money supply, price indexes, foreign investment, imports/exports, foreign aid, exchange rates, interest rates; labor force (sizes, composition, occupational distribution); democracy measures, defense expenditures, military expenditures, assassinations, coups, general strikes, guerrilla warfare, government crises, purges, riots, revolutions, antigovernment demonstrations, seats in the legislature, legislature size, legislative effectiveness, competitiveness of nominating process, party coalitions, party legitimacy, type of regime, major constitutional changes, cabinet size, Freedom House political rights index, Freedom House civil liberties index, historical conflict information, regime durability; school enrollment, illiteracy rate, pupil-teacher ratios, public education expenditures; safe water access, natural disasters, cropland area, crop yield, fertilizer consumption, rail mileage, forest and woodland information; physicians per capita, mortality rate, life expectancy, fertility rate, information on AIDS, standard of living index, birthrate, death rate; crime statistics; newspaper circulation; modernization and postmodernization indexes, cultural zone, and a subjective well-being score.

Appendix 2: A Brief Introduction to Neural Network Models

Binary outcome variables are modeled with a Bernoulli probability distribution, which means simply that the outcome variable Y takes on the value $Y = 1$ (for state failure) with probability π and $Y = 0$ (no failure) with probability $1 - \pi$. The only assumptions so far are that the definition of the two categories are mutually exclusive and exhaustive, both of which hold automatically because of the definition of state failure.

The question to be answered by statistical analysis is what the probability of state failure (π) is for possible configurations of values of the explanatory variables, x. In logistic regression, π is assumed to vary with x according to a logistic function, $\pi = 1/(1 + e^{-\alpha - x\beta})$. That is, if we plotted x horizontally and π vertically, the logistic function would look like an escalator with π approaching 0 at the bottom and 1 at the top. The direction (from top left to bottom right or top right to bottom left) and steepness of the escalator-shaped curve are determined by the values of the parameters α and β.

Choosing a logistic regression model is precisely the choice of a specific *family* of possible curves, specific *members* of which are determined by the values of α and β. Logistic regression analysis then involves

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using data to find the single member of the family (indexed by the values of α and β) that best fits the data. The procedure for choosing a member of a family, known as maximum likelihood estimation,[57] is well known and optimal in many relevant respects. The parameters α and β are of no direct interest, but they can be used to compute quantities of interest such as the probability of state failure.

The neural network model we use is directly analogous to logistic regression: it too specifies a (parametric) family of curves and then uses data to choose the particular member of the family by setting values of the parameters that best fit the data. However, instead of a curve that looks like one of many possible escalators, the possible shapes are now far more numerous and diverse (escalators, roller coasters, linear, quadratic, cubic, and many others). Figure 3 gives some examples of the possible shapes these functions can take. The neural network family of curves also includes members with considerably more intricate interaction effects (that is, when the effect of one explanatory variable depends on the values of others), as evidenced by the differences in the two columns of Figure 3.

To understand the mathematical form, note that the logistic form can be simplified and written as $\pi = \text{logit}(\alpha + x\beta)$ or even $\pi = \text{logit}(\text{linear}(x))$. The specific mathematical form of the neural network we use is then

$$\pi_i = \text{logit}\,[\gamma_0 + \gamma_1 \text{logit}(X_i\theta_{(1)}) + \gamma_2\,\text{logit}(X_i\theta_{(2)}) + \ldots + \gamma_M \text{logit}(X_i\theta_{(M)})], \quad (1)$$

or more simply $\pi_i = \text{logit}(\text{linear}(\text{logit}(\text{linear}(X_i))))$, where M (which is known as the number of "hidden neurons," but of course nothing is hidden and M has no necessary relationship to neurons in the brain), the γs and the θs are adjustable parameters that index particular members of the family. They can be estimated from the data via the same maximum likelihood procedure as for logit. Like logit, the parameters estimated are not of direct interest, but they can be used to compute forecast probabilities and other quantities of interest.

The advantage of neural networks is that they assume less as part of the choice of family and leave the data to guide us more. Indeed, the model in equation 1 can approximate arbitrarily closely any relationship between π and x, given a large enough value for M. In practice, neural networks have been found to approximate a larger range of functional forms with fewer parameters than other relevant possibilities.

The potential disadvantage of neural networks is that they are so

[57] Gary King, *Unifying Political Methodology* (Ann Arbor: University of Michigan Press, 1989).

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flexible that they can fit idiosyncrasies in the data rather than structural features that persist, although we guard against this with the methods offered in Section IV. In only rare situations will a neural network model be outperformed by a logistic model. Indeed, choosing a logistic model over a neural network model would be appropriate mainly if there existed strong substantive theory that ruled out all members of the neural network family of curves other than the logistic. In fact, however, we know of no such substantive theory of state failure that even addresses these issues and so there exists little justification for logit over neural networks. See King, Beck, and Zeng[58] for more details along the same lines and Bishop[59] for a complete presentation of neural networks.

[58] Beck, King, and Zeng (fn. 35).
[59] Bishop (fn. 38).

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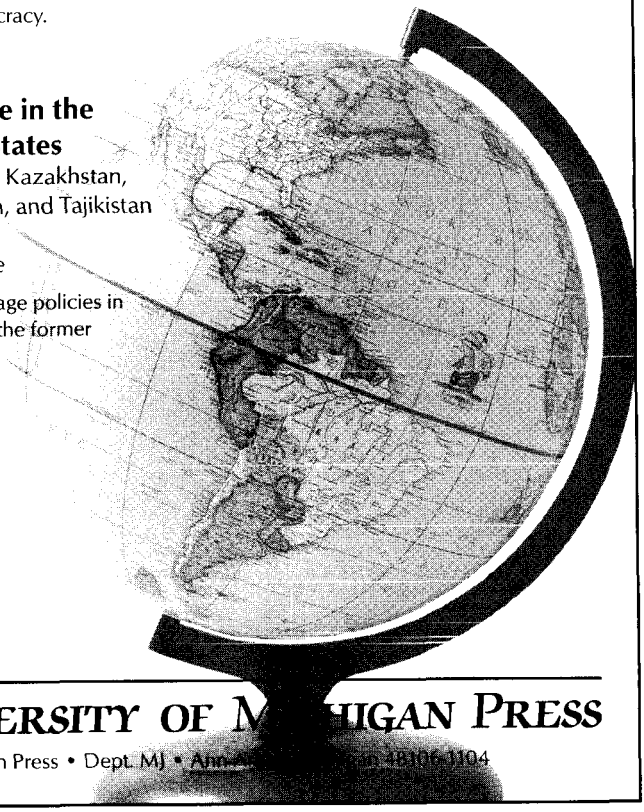

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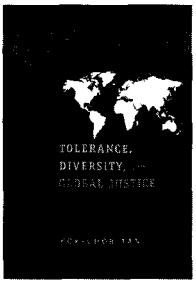

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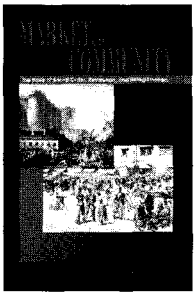

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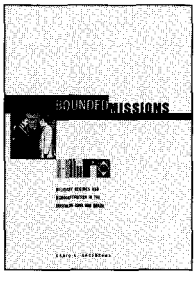

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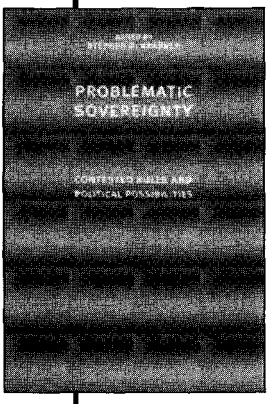

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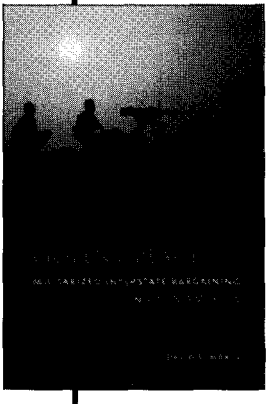

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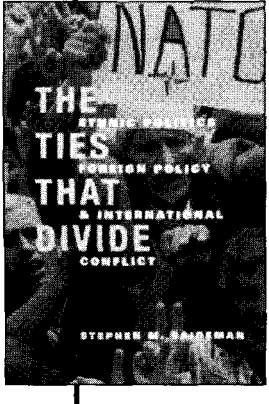

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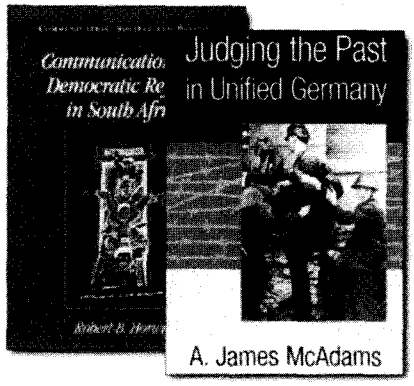

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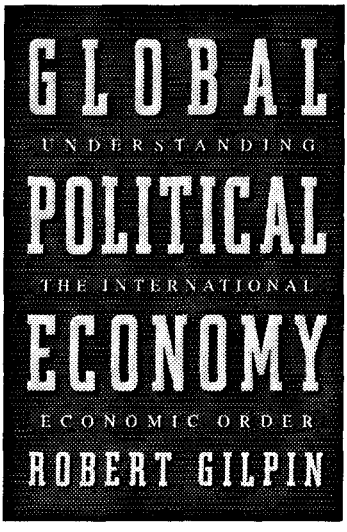

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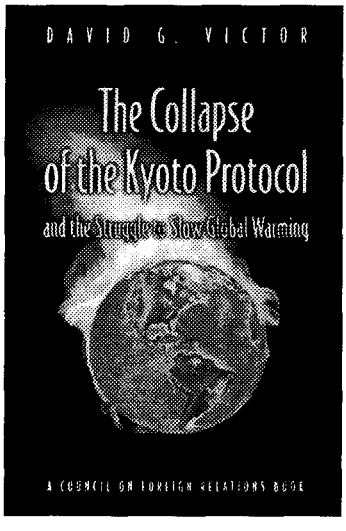

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